THE
PHILOSOPHICAL MAGAZINE
AND JOURNAL:

COMPREHENDING

THE VARIOUS BRANCHES OF SCIENCE,

THE LIBERAL AND FINE ARTS,

GEOLOGY,

AGRICULTURE,

MANUFACTURES AND COMMERCE.

BY ALEXANDER TILLOCH,
M.R.I.A. F.S.A. EDIN. AND PERTH, &c.

"Nec aranearum sane textus ideo melior quia ex se fila gignunt, nec noster vilior quia ex alienis libamus ut apes." JUST. LIPS. *Monit. Polit.* lib. i. cap. 1.

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THE

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THE

PHILOSOPHICAL MAGAZINE AND JOURNAL.

I. *An algebraical Expression of the Values of Lives, with a Mode of finding the correct Value of any Number of joint Lives.* *By* A CORRESPONDENT.

To Mr. Tilloch.

SIR,—THE bills of mortality for the year 1815, published in the newspapers of yesterday, have suggested to me the following calculations, which perhaps, from their novelty, you may think worthy a place in your Magazine.

I am, sir,
Your very obedient servant,

London, Jan. 2, 1816. A. B. C. D.

IN making calculations from the registers of the duration of lives, it has not been usual to attempt to represent the results of a whole table by a single formula, although such a simplification would afford great advantages in the solution of a variety of problems. As an instance of this mode of treating the subject, we may take the bills of mortality for the year now elapsed, and by reducing the numbers into the form of a diagram, and observing the different flexures of the curve resulting from them, we may ascertain the nature of the terms proper to constitute the required expression; and calling the age x, the annual deaths will appear to be $\frac{1}{4} \cdot \frac{1}{1+xx} + \cdot000401x - \cdot0000042x^2$, without any very material error: at least with incomparably greater accuracy than is obtained by the rough approximation of supposing an equal annual mortality in a given number of persons as long as any of them remains alive; and much more nearly than the registers of a metropolis agree with those which have been kept in the country. It would be very easy to correct the expression from the average of the registers of a number of years; and the formula might also be adapted with little difficulty to the mortality observed in any other situation.

In order to find the number of persons who have died during any given portion of the period of the longest human life, we must combine the expression for the mortality with the fluxion $\dot{x}$ of the age; the fluent, $\frac{1}{4}$ arc tang. $x + \cdot 0002005x^2 - \cdot 0000014\,x^3$, will indicate the aggregate amount of deaths at the given age. It has sometimes been thought sufficiently accurate to estimate the value of a life by the period at which half the persons who have attained the given age are likely to remain alive: this affords a true criterion of the greatest probability of the duration of one life, but not of the correct mean value of a number of lives; since this value can only be found by dividing the aggregate duration of all the lives in question by their number: now this aggregate duration is represented by the area of the curve having the ages and the numbers living, for its ordinates and abscisses; and this area is obtained from the fluent $x - \frac{1}{4}x$ arc tang. $x + \frac{1}{8}\mathrm{HL}\,(1+x^2) - \cdot 00006683\,x^3 + \cdot 00000035\,x^4$. Hence we may calculate the mean value of life in London, as shown in the following table. At birth, the most probable duration is about 27 years; the mean value more than 30.

AGE.	DEATHS.		MEAN VALUE.	HALLEY.	$43-\frac{1}{2}x$.
	Registered.	Calculated.		(BRESLAU.	
0	000	·0000	30·29		43
1		·1954	36·53	33·5	$42\frac{1}{2}$
5	385	·3480	40·80	41·2	$40\frac{1}{2}$
10	409	·3864	38·21	40·4	38
20	444	·4492	31·80	34·2	33
30	517	·5269	26·60	27·9	28
40	610	·6177	21·46	22·3	23
50	717	·7140	16·96	17·2	18
60	813	·8080	12·90	12·4	13
70	896	·8914	9·09	7·6	8
80	959	·9559	5·17	4·5	3
90	994	·9933	1·64		
95		·9993			
100	999·9				

The advantage of this method of calculation is strongly exemplified in the determination of the value of the joint continuance of two or more lives, which may be obtained by means of the quadrature of a curve having its ordinates in the joint ratio of the survivors at the time expressed by the absciss; the corrected area being divided by the product of the numbers of survivors at its commencement, which obviously expresses the number of all the possible combinations of their lives, as the area does the aggregate duration of those combinations.

For this purpose it will be convenient to represent the number of deaths by the formula $\cdot 38 + \cdot 002x^2 - \cdot 00000137x^3$, which will be sufficiently accurate for any age exceeding 10 years; and if we make $a = \cdot 62$, $b = \cdot 0002$, and $c = \cdot 00000137$, and call the two ages x and $x+n$, the product will be $d + ex + fx^2 + gx^3 + hx^4 + ix^5 + kx^6$, where $d = a^2 - abn^2 + acn^3$, $e = 3acn^2 - 2abn$, $f = b^2n^2 - 2ab + 3acn - bcn^3$, $g = 2b^2n - 4bcn^2 + 2dc + c^2n^3$, $h = b^2 - 5bcn + 3c^2n^2$, $i = 3c^2n - 2bc$, and $k = c^2$; and the area of the curve will be $dx + \frac{1}{2}ex^2 + \frac{1}{3}fx^3 + \frac{1}{4}gx^4 + \frac{1}{5}hx^5 + \frac{1}{6}ix^6 + \frac{1}{7}kx^7$, which must be found for the given age, and for $x + n = 98$ or 100, and the difference, divided by the product, will show the value of the joint lives.

But in pursuing the calculation for a greater number of lives, it would be necessary to assume a still simpler expression for the number of deaths, such as mx, m being $\frac{1}{86}$, or from $\frac{1}{80}$ to $\frac{1}{90}$ according to circumstances, retaining the more accurate expression for the elder lives; and taking for the age of the younger $x-p$, and for the number of the survivors $1 - mx + mp$, which may be called $q - mx$: and the former product might be multiplied by this for the case of three lives, and the area found as before Indeed this expression may be employed for the younger of two lives without material inaccuracy, the product becoming $aq - amx - bqx^2 + (cq + bm)\,x^3 - cmx^4$, and the area $aqx - \frac{1}{2}amx^2 - \frac{1}{3}bqx^3 + \frac{1}{4}(cq + bm)\,x^4 - \frac{1}{5}cmx^5$; whence, for example, when the ages are 10 and 20, supposing $m = \cdot 012$, we obtain the mean joint value 22·9: nor would the result in this case be materially different if we employed the same simple estimate of the mortality with respect to both lives, though it would vary more at other ages. We may however safely make the value of $m = \cdot 012$ between the ages of 20 and 60 in London, even for the case of three joint lives, the number of survivors being called $1 - mx$, $1 + mp - mx$, or $q - mx$, and

$1+mr-mx$, or $s-mx$, respectively; p and r being the differences between the eldest and the two younger lives: the product of these will be $qs-m(s+q+qs)x+m^2(1+q+s)x^2-m^3x^3$, and the area $qsx-\frac{1}{2}m(s+q+qs)x^2+\frac{1}{3}m^2(1+q+s)x^3-\frac{1}{4}m^3x^4$, which must be found for $mx=1$, and for the given age, and the difference divided by the product.

When two lives are equal, the mean value of their joint continuance, thus approximated, becomes exactly two-thirds of that of a single life; of three, two-fourths or a half; of 4, two-fifths, of 5 two-sixths, and so forth: whence also we obtain, for the value of the longest of two lives, $\frac{4}{3}$ of that of a single life, and for the longest of three $\frac{6}{4}$; and we may continue the series at pleasure by adding at each step 2 to the numerator and 1 to the denominator.

According to the usual method of estimating the value of three joint lives from that of two lives, one of which is of equal value with two of the three taken together, the result, in the case of equal lives, is about $\frac{14}{27}$ of the value of a single life, instead of half; that is, almost 4 per cent. too much, an error by no means to be neglected in practice. It will be easy to obtain a more correct approximation, from the principles here explained, employing any tables of the value of lives that may be preferred. Let m be found for the eldest life, by making $\frac{1}{m}$ equal to twice its value, increased by the age: and let t and u be found in the same manner for the other two lives, so that the numbers of survivors may be denoted by $1-mx$, $q-tx$, and $s-ux$, q being $1+tn$, and s $1+ur$: the area will then be (q[illegible]$-\frac{1}{2}(qsm+qu+st)x+\frac{1}{3}(tu+qmu+smt)x^2-\frac{1}{4}mtux^3)x$, which must be taken for the given age of the eldest life and for $mx=1$, and the difference divided by the product of the survivors will give the value of the three joint lives, with much greater accuracy than it can be determined in the manner directed in the Legacy Duty Act.

This remark is, however, only strictly correct, with regard to the precise amount of the error in question, when the age is so great, that the different effects of the operation of interest on the relative pecuniary values of the lives may be disregarded. It is obvious that the preceding calculations are wholly independent of this consideration, giving us only a theoretical mean value, from which it is not possible to deduce immediately a practical value, without further reference to the comparative numbers of the survivors at different ages, in order to estimate the various operation of interest, and particularly of compound interest. But the same mode of computation may readily be extended, so as to comprehend the effect of interest also, by supposing the ordinates, expressing the number of survivors, to be reduced in the proportion of the present value of a given sum payable at the corresponding time: that is, by multiplying the expressions denoting them by v^x; v being the present value of a unit payable at the end of a year.

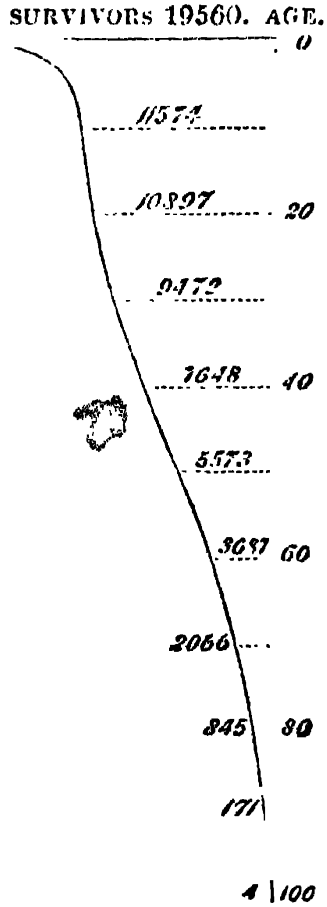
But the expression containing the circular arc becomes intractable, even when applied to the termination of the present value of a single life: we may therefore take, instead of the primary expression $\frac{1}{1+xx}$, some members of the equivalent series $\frac{1}{x^2} - \frac{1}{x^4} + \frac{1}{x^6} \ldots$, and make the fluxion $\frac{1}{4}\dot{x}\left(\frac{1}{x^2} - \frac{1}{x^4}\right)$, of which the fluent is $\mathrm{C} - \frac{1}{4x} + \frac{1}{12x^3}$; and this, if we put $\mathrm{C} = \cdot 405$, will be sufficiently accurate even in infancy. We shall then have, for the number of survivors at the age x, $\cdot 595 + \frac{1}{4x} - \frac{1}{12x^3} - \cdot 0002005x^2 - \cdot 0000014x^3$; which, multiplied by $v^x\dot{x}$, will give the fluxion of the area.

The fluent may be found by means of the following theorems of Euler's *Calculus Integralis* (§ 228, 225, 224); $\int\frac{v^x\dot{x}}{x} = \mathrm{HL}x + x\mathrm{HL}v + \frac{x^2(\mathrm{HL}v)^2}{1.2.2} + \frac{x^3(\mathrm{HL}v)^3}{1.2.3.3} + \frac{x^4(\mathrm{HL}v)^4}{1.2.3.4.4} \ldots (= \mathrm{X})$; $\int\frac{v^x\dot{x}}{x^3} = \frac{(\mathrm{HL}v)^2}{2}\mathrm{X} - \frac{v^x}{2x^2} - \frac{v^x\,\mathrm{HL}v}{2x}$; $\int v^x\dot{x} = \frac{1}{\mathrm{HL}v}v^x$; $\int v^x x^2\dot{x} = v^x\left(\frac{x^2}{\mathrm{HL}v} - \frac{2x}{(\mathrm{HL}v)^2} + \frac{2}{(\mathrm{HL}v)^3}\right)$; and $\int v^x x^3\dot{x} = v^x\left(\frac{x^3}{\mathrm{HL}v} - \frac{3x^2}{(\mathrm{HL}v)^2} + \frac{3.2x}{(\mathrm{HL}v)^3} - \frac{3.2}{(\mathrm{HL}v)^4}\right)$. When x is large, the series converges somewhat slowly: but its utility is chiefly confined to the earlier ages; and the ultimate value, being once only computed, for $x = 100$, will serve for the correction in all other cases: but after the age of 10, the formula $a - bx^2 + cx^3$, already mentioned, will be more convenient. The fluent, when corrected, by subtracting it from its ultimate value, must be divided by the number of survivors, and multiplied by $\frac{1}{x}$, since it would otherwise express the value of the continued payment at the time of birth, instead of the actual present value.

The same solution may be applied to the expressions for the value of joint lives, multiplying always the product of the survivors by $v^x \dot{x}$; the fluents being all comprehended in the general theorem $\int v^x x^n \dot{x} = v^x \left(\frac{1}{\text{HL}v} x^n - \frac{n}{(\text{HL}v)^2} x^{n-1} + \frac{n(n-1)}{(\text{HL}v)^3} x^{n-2} - \frac{n(n-1)(n-2)}{(\text{HL}v)^4} x^{n-3} + \ldots \right)$. For the joint present value of three or more lives, the quantities m, t, and u, may be determined from the theoretical mean values of each, or otherwise, and the present value, thus approximated, will differ much less from the truth than the theoretical mean values would do, if originally found in a similar manner. The usual deduction must be made for periodical payments, whether annual or quarterly: and a slight correction may sometimes be required, on account of the different effects of annual and continual interest; but both these points are very easily arranged: commonly, indeed, the subtraction of half a payment from the present value, thus computed, will give us the corrected value with considerable accuracy.

The great labour required for such calculations, according to the usual methods, renders it very difficult to adapt the tables of annuities to every possible variation of the value of life; the improved habits of society, and probably also the advancement of the medical sciences, and especially the introduction of vaccination, seem to have effected by degrees a very material change in the mortality of this metropolis; and it appears that the magnitude of such a change, and its operation, in its various modifications, may in many cases be much more conveniently appreciated by this mode of finding a continuous law for the decrements of life, than by the inspection of tables, and the numberless combinations of their elements.

Mortality of London 1815.



II. *On the Cosmogony of Moses.* *By* A Correspondent.

To Mr. Tilloch.

Sir,—Like your correspondent Homo[*], I have been unable to discover, among the merits of Cuvier's Essay on the Theory of the Earth, that accordance with the Mosaic cosmogony which the preface of Professor Jameson had taught me to anticipate. I confess, too, that this accordance has not been rendered much more perceptible to my understanding, either by Homo's proposed reading of the beginning of Genesis, or by the subsequent observations of Dr. Prichard[†], in support of the term *day* being, in the *Mosaic account*, equivalent to an *indefinite period*.

"In the beginning God created the heaven and the earth: and the earth was without form, and void, and darkness was upon the face of the deep." If it be supposed, with Homo, that this act of divine energy was an indefinite period antecedent to what is called *the six days' creation*, there will, as he observes, be no scriptural objection to the adoption of the belief, that the earth may have "*thus existed*" the "thousands of years that preceded the history of man." Homo's *reading* thus affords time for the deposition of the primitive rocks, as well as for whatever else may have taken place previous to the gathering together of the waters, and the appearance of the dry land on the third day; but there its explanatory power expires, leaving Cuvier's[‡] "*thousands of animals that never were contemporaneous with man*," together with his various deluges, to be reconciled to the Mosaic account by some other hypothesis.

As what Homo calls *Bishop Horsley's hypothesis* of a *slower revolution* may by some be deemed sufficient for this purpose, I must beg leave to offer a few observations relative to it. In the preface already spoken of, Professor Jameson says, that "there are indeed *many physical considerations* which render it *probable* that the motions of the earth may have been slower during the time of its formation than after it was formed[||], and consequently that the *day*, or period between morning and evening, may have then been indefinitely longer than it is at present." From this passage there is the reference I have given in the margin, to Bishop Horsley's Sermons. To them I turned, naturally expecting to find a development of the announced hypothesis; but instead of the "*many physical considerations* which render it *probable*," I found only a well-reasoned refutation of the conjecture that, in the Mosaic history of the creation, the word *day* means an indefinite portion of time. Towards the close of the argument, the bishop says, "By what description could the word *day* be more expressly limited to its literal and common meaning, as denoting that portion of time which is measured and consumed by the earth's revolution on her axis? That this revolution was performed in the same space of time in the beginning of the world as now, I would not over confidently affirm." This last is the only sentence in the sermon, that connects Bishop Horsley's name with the supposition of *a slower revolution*; which I shall now consider a little on the ground of its intrinsic value. The rotation of the planets on their axis seems exempt from the accelerations and retardations to which all their other motions are subject. No inequality has ever been detected in the rotary motion of the earth; and the relation between its polar and equatorial diameters shows, that when it assumed its present figure the centrifugal force of its particles, and consequently the rapidity of its diurnal revolution, could not be much different from what they are at present. But it may be said, that when the rotary impulse was given, the matter of the earth constituted the shell or crust of a hollow sphere; that afterwards this crust, being broken, fell in towards its centre, and formed a spheroid of diminished magnitude; and that, in consequence, the rotary motion of the spheroid must (in a certain proportion to its diminution) be more rapid than the original diurnal revolution of the hollow sphere. That an acceleration of the earth's rotation must have resulted from such a catastrophe is mathematically demonstrable; but besides that the supposition is wholly gratuitous, it would give no effectual support to the hypothesis, unless a magnitude were given to the original hollow sphere, which the most visionary imagination would scarcely dare to assign. Admitting, however, for a moment this hypothesis of *a slower revolution*, I would ask, Of what duration was the earth's primitive diurnal rotation? Was it one thousand, or one hundred, of our years? Although the greatest of these periods would, perhaps, be too short for the purpose assigned it, let us take only half of the least, and inquire what would be the state of the vegetable and animal world deprived of *light* and *heat* during *five-and-twenty years?* for such, it would appear, must have been their condition, at least after the *fourth day*, on the supposition of the diurnal revolution of the earth being equal to no more than fifty of our years.

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[*] Phil. Mag. No. 209.

[†] Ib. No 210.

[‡] Cuvier's Essay, concluding period.

[||] Vide Bishop Horsley's Sermons, 2d vol. pages 245 and following. In the preface the reference is to "p. 445 second edition," owing, I suppose, to a mistake.

There remains to be noticed the conjecture often made, that in the Mosaic cosmogony the word *day* signifies an indefinite portion of time. This conjecture seems completely disproved by Bishop Horsley: but with relation to Dr. Prichard's argument in its favour, drawn from the genius of the Hebrew language, I would ask, whether the Hebrews ever understood the word *day*, as used in the first chapter of Genesis, to signify an indefinite portion of time? If they did not, is it probable that the true sense of this Hebrew word, as there employed, should always have remained unknown to them, and after a lapse of some thousand years, when they had long ceased to exist as a nation, be discovered by the ingenuity of modern criticism? Acquiescing in the correctness of the remark, that the Hebrew word which we translate *day*, was often used by the Hebrews, and even by Moses himself, to designate an indefinite length of time, the just inference seems to be, that, on account of this usage of the term, Moses, when speaking of the Creation, thought it necessary to circumscribe the length of each *day* by its natural boundary. To show therefore that *day* used unrestrictedly, was sometimes understood by the Hebrews to signify an indefinite period, instead of strengthening Dr. Prichard's conclusion, tends directly to its subversion; unless he can further show, that the word in Hebrew was ever so understood when the extent of its signification was expressly limited to the duration of an *evening and a morning*, or to the *decay of light and its return*, as Bishop Horsley observes the words of Moses literally import.

An auxiliary argument of Dr. Prichard's is, that it would be imputing a palpable absurdity to Moses to suppose him speaking of days in the literal meaning of the word, before the creation of the sun. But may it not be asked, whether the imputation of absurdity would be greatly diminished by Dr. Prichard's alternative of making him include an indefinite period of great length, *in an evening and a morning?* In this view, therefore, the assumed figurative sense does not seem to have much advantage over the literal. With the indulgence, however, of a little of the privilege of conjecture, so freely used by others on this subject, it will not be difficult to clear the *literal sense* from its apparent inconsistency.

Darkness being merely the absence of light, it is impossible that they could ever coexist in the same place, so as to be subject, like substances in union, to an actual division. When, therefore, after the creation of light, Moses says that God divided the "light from the darkness," he cannot be literally understood: but we may suppose the matter of light at the moment of its creation diffused through the immensity of space previously occupied by darkness, and that the dividing the light from the darkness consisted in collecting it into one great temporary reservoir. Now after this aggregation of the matter of light on the first day, provided its position were not in the prolonged

longed axis of the earth, the earth's rotary revolution would be as accurately marked, and the appearances of day and night, of the decay and the return of light, would as regularly succeed each other as after the creation of the sun.

I am, sir,
Your very obedient servant,
Bath, Dec. 12, 1815. F. E---s.

III. *Geological Queries, regarding the Coal-Strata of Northumberland and Durham, and the Appearances of such in Lincolnshire. By* A CORRESPONDENT.

To Mr. Tilloch.

SIR,—IT is with great pleasure that I recognise in pages 465 and 6, (though under the signature N. J. W.) a communication "on the localities of coal," to be from the able pen of *Nathaniel John Winch*, esq., who so very kindly answered former queries put by me on the same subject, in pages 198 and 295 of your xlvth volume; and I am by this circumstance emboldened, to again trouble him as follows; viz.

1st. Has it been ascertained, by actually *tracing the Rocks* and strata on the surface (as *Mr. Smith* has done in his Map), or by a sufficiently extensive and careful comparison of *the successions of beds and organic remains* therein imbedded, in the distant situations of Alston-Moor, and others W of Bywell, and N of the Coquet River, that the individual thin and nearly *useless seams* of Crow Coal* in the former situation, thicken and improve in quality, so as to become in the latter and more northerly situations, tolerably good seams of *workable Coal?*

2d. Do the over-lying or mountain caps of Basalt in the northern and western parts of Northumberland, appear to be *unconformable*† to the Strata beneath them ?, that is, do they lie upon the edges of their substrata, and have no place *in* the series of strata to the east of their sites ? Or, are they detached parts or hummocks of the great whin sill (Forster fath. 423 to 437)?: whose continuous basset-edge would in such case be traceable, from Dufton-fell and Caldron-snout (p. 41), north-eastward to the sea.

3d. Can it be depended on, that small shells of *Muscles* (whether of *salt* or *fresh* water origin, I do not ask, because this, perhaps, no one can safely say) " are met with *in every Colliery*" of this district (p. 466); or even " in most places where iron-stone is found in the shales, not only in the Newcastle Coal-formation, but also in the metalliferous Limestone district, upon which it rests," (p. 364 vol. xlv.)?

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* Of which Mr. W. Forster enumerates seven distinct beds, in the Lead-mining series, from fathom 244 to fathom 602 of his Section.

† Are there in these districts, any masses of Red Marl or soft red Sandstone, accompanying these Basaltic Caps, or otherwise, in an *uncomformable* manner?

A particular answer, mentioning as many *places* (and if practicable the *strata* also, with reference to Forster's Section) as possible, where such shells have been actually found, will infinitely more oblige the querist, than any general answer whatever to this question.

4th. In the boreing for Coals at Dinsdale on the Tees (p. 466), were any real Coal-seams, however thin, actually penetrated? or were ever real Coal-measures or metals, proved in this place?.

The querist hopes his solicitude on this head may be pardoned by Mr. W., when he mentions, that to him, when some years ago hastily viewing this district, Dinsdale seemed to be situated above the magnesian Lime, on a part of the British Series, wherein no Coal-seams have yet to his knowledge been proved.

5th. Was it *near Wainfleet* on the Lincolnshire Coast, that fragments of a black (brown?) Coal resembling that of Bovey were washed on shore, together with bituminous Shale containing numerous Shells? The querist has been induced, for two reasons, thus to frame his question; first, because Mr. W. intimates, that the same kind of shale is *a substratum* to the Chalk, which the strata S of Wainfleet must be; and next, because the foot of the hill on the west side of Bolingbroke in the range of these strata, produces such gray, bituminous, shelly, shale, and near thereto on the NE of Rathby, they were boreing in search of Coals, at the time he visited this district, *in the clunch Clay*; the same part of the series of British strata, in which, at another time, he found the thin Coal-formation of the Yorkshire east moorlands (p. 466) to be situated. Whereas *Louth* appeared clearly to him to stand, on *the top of all the Chalk Strata*: if therefore any such beds of shale resembling Coal-measures, especially seams of Coal, were there found, in or near to the London Clay (on which Alford stands), it would prove a fact quite new to the querist: except perhaps, that he once heard, without giving credence thereto, that Coals appear above the Chalk, somewhere to the east of the Medway near the southern shore of the Thames?

Not doubting Mr. W's friendly reception of the above queries and hints, or that the answers to be expected thereupon will interest many,

I remain,

Jan. 2, 1816.

A CONSTANT READER.

IV. *On*

IV. *On Micrometer Telescopes.* *By* Ez. Walker, *Esq. of Lynn, Norfolk.*

To Mr. Tilloch.

Dear Sir,—In Dr. Brewster's ingenious treatise on new philosophical instruments, there is a note on page 59, respecting my micrometer, described in the xxxviiith volume of the Philosophical Magazine, which is not perfectly correct. I have, therefore, to request the favour of a place in your Magazine, to state the subject more explicitly than it seems to have appeared to the learned author.

On page 59, Dr. Brewster gives "A Description of an Eye-piece Wire Micrometer," with the following paragraph as a note:

"An instrument of nearly the same kind (says Dr. Brewster) with that which is the subject of the following chapter, has been described by Mr. Ezekiel Walker, in the Philosophical Magazine for August 1811, vol. xxxviii. p. 127, as a new invention of his own. I certainly cannot suppose that this ingenious writer had an opportunity of seeing any of the instruments of this kind which had been constructed under my direction. So early as the end of the year 1805, I sent a drawing and a description of the eye-piece micrometer to Mr. Cary, optical-instrument maker, London. In 1806, one of the instruments made for me by Messrs. Miller and Adie, Edinburgh, was examined by Professor Playfair; and since that time it has been in the possession of a friend in London. The only variation in the instrument proposed (*constructed*) by Mr. Walker, is the use of lines on a slip of mother-of-pearl, instead of the silver wires."

Now the Doctor's micrometer (if I understand it) is founded on the following property of the telescope, viz. when the eye-tube, containing two glasses, is drawn out, the magnifying power of the instrument is increased; and this is the same principle on which mine is constructed. This property seems to have been discovered by Dr. Brewster himself; but it is not new, for an account of it was published many years ago.

In the year 1771, the late Mr. Benjamin Martin published a description of a new optical instrument which he calls "*Microscopium Polydynamicum:* or a new Construction of a Microscope, wherein a variety of magnifying powers is communicated to each object-lens."

This microscope with its description I had from Mr. Martin's shop, No. 171, Fleet-street, London. Consequently, I have been many years acquainted with this property in optics, which affords a very pleasing variety in the use of microscopes and telescopes.—Of Dr. Brewster's instrument I knew nothing, till I saw an account of it, a few days ago, in his book.

In the Doctor's note it appears, that I use lines on a slip of *mother-of-pearl,* in my instrument: but this is a mistake; for it is expressly mentioned in my paper, that my micrometer consists of a number of parallel lines drawn upon a piece of *plane glass.*

Soon after I had formed the design of this instrument, I applied to the late Mr. Coventry of Southwark, for a set of his micrometers. Mr. C. had none at that time, but promised to send me a set, which I never received though I waited for them upwards of two years. But, not being willing to give up the plan of my instrument, without trying how far it might be rendered useful in practice, I undertook to draw lines upon glass myself; and after an infinite number of unsuccessful attempts, I succeeded so far as to draw parallel lines $\frac{1}{1000}$th of an inch apart, which being intersected at right angles by equal lines form minute squares, each one-millionth part of a superficial inch. Whence I concluded that lines drawn, by the same method, at $\frac{1}{100}$th of an inch distant would be sufficiently accurate for my purpose.

I am surprised to find so little use made of Mr. Coventry's glasses; for, if they were properly applied to optical instruments, micrometers might be made that would supersede the use of all others. For measuring small objects before the microscope, these micrometers are unquestionably the best, both for precision and readiness of application; and they may be applied to telescopes for measuring small angles with equal advantage. To illustrate this position, let the second example be taken from p. 128, vol. xxxviii. of The Philosophical Magazine.

"The magnifying power of the telescope is 45 times.

"Micrometer divisions $\frac{1}{100}$th of an inch distant.

"Each division subtends an angle at the eye = 30″.

"One inch of the eye-tube is .. .. = 52″."

But if the divisions of the micrometer were drawn $\frac{1}{200}$th of an inch distant, which may be done with great exactness: Then,

Each division would subtend an angle .. $= \frac{30}{2} = 15''$.

And one inch of the eye-tube would be .. $= \frac{52}{2} = 26''$.

Consequently,

$\frac{1}{10}$th of an inch on the eye-tube would be .. .. .. $= \frac{26}{10} = 2{\cdot}6''$

$\frac{1}{100}$th of ditto .. would be $= \frac{26}{100} = 0{\cdot}26''$

$\frac{1}{200}$th of ditto .. would be $= \frac{26}{200} = 0{\cdot}13''$

On the Micrometer.

As the divisions of the eye-tube may be read off to within $\frac{1}{100}$th part of an inch by a vernier scale, consequently the error would seldom exceed one-tenth of a second; and the errors arising from mechanism in this micrometer cannot be of much consequence, since the eye-tube is never drawn out further than $\frac{6}{10}$ths of an inch; for 2·6″ × 6 (= 15·6″) is nearly = one division of the micrometer.

From the comparative micrometer measures taken at the Observatory, Armagh, by the Rev. Dr. J. A. Hamilton, it appears that the divided object-glass micrometer and the wire-micrometer are each liable to an error of 3″ in taking the sun's diameter*.

On the divided object-glass micrometer, Dr. Hamilton observes, "its imperfections are; that, to different eyes, and under different circumstances of the same eye, the length of the focal distance, that suits distinct vision, will vary, and of course the quantity of the measures given by the scale is liable to a small variation.

"The principal defects of the wire-micrometer are; the difficulty of judging accurately of bisections, or contacts of the fine wires, by the limbs to be measured; and the impossibility of observing any diameter, except one perpendicular to the equator."

In the construction of my micrometer, I endeavoured to avoid all optical illusions, and all mechanism that might produce any sensible error.

I remain, dear sir,
With much regard,
Your very obedient servant,
Lynn, Dec. 29, 1815. Ez. Walker.

V. *On the Nature and Combinations of a newly-discovered vegetable Acid; with Observations on the malic Acid, and Suggestions on the State in which Acids may have previously existed in Vegetables. By M.* Donovan, *Esq. Communicated by* W. H. Wollaston, *M.D. Sec. R.S.*

[Concluded from vol. xlvi. p. 444.]

Observations on the Malic Acid.

In 1785, during an examination of different fruits and berries, Scheele discovered that gooseberries, beside lemon acid, contained one of a peculiar nature: this he afterwards found to exist in apples, without, as he thought, a sensible admixture of any other. On this account he gave it the name of apple acid, or malic acid.

He also ascertained, that by the action of nitrous acid on sugar, a substance is produced, which shows no traces of nitric acid, yet unites and forms a soluble salt with lime, "it therefore is not the oxalic acid." By some other experiments he found that an acid is produced "which does not differ in the least from the properties of the apple acid, and is accordingly the same."

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* Irish Transactions, vol. x.

This acid he detected in a great variety of vegetable juices. Since that period, Vauquelin has extended the catalogue; but of all other plants, it is most abundantly contained in the *Sempervivum Tectorum*.

Scheele's process for obtaining malic acid is as follows. "Saturate the juice of apples, whether ripe or unripe, with carbonate of potash; add solution of acetate of lead until it cease to produce a precipitation. To the edulcorated precipitate, add as much dilute vitriolic acid, as is necessary to give the mixture a perfectly acid taste, without any sweetness*."

There are several objections to this process, all of which seem to have considerable weight. In the preceding pages I have shown, that the juice of apples, whether ripe or unripe, always contains two acids of very different properties. By the above process these acids are not separated; they are in fact found in what is supposed to be the resulting pure malic acid, and it is impossible, without the most complicated processes, to obtain this substance in the insulated form.

The precipitation of the lead by means of sulphuric acid appears to be objectionable. I have often attempted to adjust the proportion of the latter substance, so as to throw down all the lead, without leaving any free sulphuric acid but I uniformly failed: and it is evident that, if not impossible, it is exceedingly difficult and troublesome.

Scheele also attempted to obtain malic acid from malate of lime, by means of sulphuric acid, but found "the mode rather difficult, as the acid would not let the calx fall completely." Vauquelin observed the same thing.

The last process proposed by Scheele, is to distil equal parts of weak nitric acid and sugar, until the mixture become brown, which is a sign that all the nitrous acid has been abstracted: the oxalic acid formed, is to be separated by lime-water, and the remaining acid to be saturated with carbonate of lime. The solution is to be filtered, the filtered liquor to be mixed with alcohol, and the coagulum thus obtained, is to be edulcorated with new portions of alcohol. The coagulum is then to be dissolved in water, and mixed with a solution of acetate of lead: a precipitate falls, which is to be treated with sulphuric acid, in the manner already directed.

No one who has not gone through this process, can fully conceive the difficulty and expense of it: and I have found that the acid obtained is variable in its nature. In one case I obtained an acid, which, when mixed with solution of acetate of lead, did not at first produce any effect, but at length slowly deposited a precipitate. The heating of another portion of the acid with carbonate of lime, produced a separation of a black powder, which possessed the properties of charcoal. There were also many other peculiarities; and the combined effect of all was to convince me, that great differences exist between the acid obtained in this manner, and that obtained by other processes.

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* Crell's *Chemische Annal.* 1785. vol. ii 295.

The experiments of Vauquelin satisfied him that the acid which is combined with lime in the *Sempervivum Tectorum*, is the true malic; and all my trials convince me, that it does not contain even the least quantity of the sorbic. Since then, by the means generally employed, we do not obtain malic acid, the only alternative is to adopt the hitherto neglected process of Vauquelin; and it will be found that his process affords the acid with greater ease, and in much greater purity, than any other. The method of detaching the acid from the malate of lead by sulphuric acid is, as we have seen, difficult; and the criterion of the taste is liable to this fallacy, that as the sourness increases, the sweetness decreases. There will, on this account, be a period when the latter will disguise the former, and yet the lead will be still present. I would therefore suggest the substitution of sulphuretted hydrogen in place of sulphuric acid.

If it were required to obtain malic acid exceedingly pure, and still more divested of vegetable matter, the following process may be adopted.

The juice of *Sempervivum Tectorum* is to be evaporated to two-thirds, and, after standing some hours, it is to be filtered, and mixed with an equal quantity of alcohol. The coagulum is to be separated by the filter, edulcorated with fresh portions of alcohol, and dried in the air, lest any adhering alcohol should impede its subsequent solution. The mass is then to be dissolved in water, mixed with solution of acetate of lead, and the precipitate collected on a filter. After being well edulcorated from any superfluous acetate of lead, the precipitate is to be boiled for fifteen minutes with a little less of dilute sulphuric acid than is sufficient to saturate the oxide of lead: and for this part of the process, the criterion of sweetness will very well answer the propose. The whole is to be set aside for some days, and, during this period, a small quantity of sulphate of lead which the malic acid held dissolved, will be deposited. The liquor is now to be filtered, and in order to separate the last portions of lead, a stream of sulphuretted hydrogen is to be transmitted through it: the black precipitate is to be filtered off, and the liquor should be boiled, in a vessel freely exposed, until paper moistened with acetate of lead is no longer blackened by the discharged vapour. This acid is the purest that can be obtained; it retains a slight odour of the gas, but even this is destroyed by exposure to the air for a few days.

Vauquelin observes, that malic acid thus obtained is nearly colourless: his was therefore diluted. I have found that it becomes perfectly brown by concentration: and I have decomposed and recomposed malate of lead several times, using each time the same specimen of malic acid; yet so obstinately did the colouring matter adhere, that it was always found in the resulting acid. Thus, as far as we know, this acid cannot be procured free from colour; and the nearest approximation is that obtained by Vauquelin's process.

Suggestions concerning the State in which Acids may previously have existed in Vegetables.

I have sometimes indulged in the supposition, that the vegetable acids are not primarily formed by the immediate union of their elements, but that they may have previously existed in a definite combination, called the bitter principle. It is possible that this principle may be a compound basis, which by uniting to oxygen, or by undergoing more complicated processes, might change its nature so far as to become an acid. The whole is a mere conjecture, and perhaps deserving of little consideration; the facts, however, which suggested it may be noticed.

The sweetness of any vegetable juice has been generally attributed to a sweet principle called sugar. In the same manner it has been lately supposed, that bitterness depends on a bitter principle, which, although variously disguised, is always identical. Dr. Thomson has shown, that when water is digested over quassia, and afterwards evaporated to dryness, a transparent substance is obtained, which differs in its properties from all other vegetable principles: this he considers as the bitter principle, and, I believe, with very great justice. I found that the liquor obtained by digestion, although slightly coloured, was transparent even to the end of the evaporation. The resulting mass was nearly transparent, and minute in quantity, considering the proportion of quassia employed; and such was its bitterness, that a particle placed on the tongue, which could not have exceeded $\frac{1}{10}$th of a grain, diffused an intense bitterness over the whole mouth and fauces.

This matter was heated with nitric acid; it dissolved with effervescence, and the bitterness was no longer sensible. The remaining substance formed a precipitate in acetate of lead, which possessed all the properties of malate of lead: and it appeared that no other than the malic acid was produced. With this experiment the following agrees in a remarkable manner.

Five ounces of white sugar, and an equal weight of very strong nitric acid were mixed in a retort. Without the application of external heat the action commenced, and soon became violent. When cold, the residual matter was found to be thick and tenacious; its taste was sour, and extremely bitter. The malic acid being abstracted from a portion of this by means of lime, it was found that the bitterness, now no longer disguised by acidity, had become intense. The other portion, which had not been saturated with lime, by being treated with more nitric acid, lost all its bitterness, and oxalic acid was formed. In this experiment it appears, that by some action of nitrous acid on sugar, a bitter substance and malic acid were produced together; that by the further action of nitrous acid, the bitter substance disappeared, and acid appeared in its stead.

The foregoing conjectures correspond also with the fact, that by the action of certain substances on each other, the bitter principle is evolved at the same time with those acids which I suppose to have been produced from that compound basis: and the appearance of both at the same time may be accounted for by admitting that the conversion was not complete. Thus, if alcohol be distilled with nitrous acid, a liquor is produced which has a sweet taste. If this liquor be re-distilled with another portion of acid, a bitter liquor comes over. And if this bitter liquor be distilled a third time with a fresh portion of nitrous acid, crystals of oxalic acid make their appearance in the residuum. This series of changes bears a striking resemblance to that produced by the action of nitrous acid on sugar.

Haussman observed, that when nitric acid is digested with indigo, a very bitter substance results, to which Welther gave the name of Amer: in this process, oxalic acid is also formed.

The vegetable acids are even formed by the action of nitrous acid on animal substances; in the instance of muscle we obtain the above-mentioned Amer with oxalic acid. In bile the bitter principle is already formed; when acted on by nitrous acid, oxalic acid is produced.

On examination we shall not be at a loss to find operations analogous to some of the preceding, taking place naturally in the vegetable kingdom. The *Pyrus Malus* or common crab-apple, while young, is very bitter, and has little sourness: as the fruit advances towards maturity, the taste becomes proportionately sour, and the bitterness diminishes. The young berries of the *Sorbus aucuparia* also are bitter, contain but one acid, and even that in small quantity: when the berry is ripe, it contains two acids, the combined quantity of both is considerable, but the bitterness has in a great degree given place to a coarse astringency.

It is not improbable, that the bitterness produced in all the foregoing cases should be owing to the formation of the same bitter principle: and its constant conjunction with a vegetable acid, seems to show that there is some very intimate connexion between them, at present unknown.

The preceding observations are offered as mere conjecture; and I am fully sensible of what little consideration should be attached to them: they are not however entirely devoid of probability. An hypothesis is below the dignity of a system which is founded on the indestructible basis of experiment: and even though it be supported by the coincidence of admitted facts, by direct analogies, and by the correspondence of received opinions, it should nevertheless be the beginning and not the end of knowledge.

VI. *Rules for ascertaining the Strength of Materials.*

To Mr. Tilloch.

Sir, — The following rules for ascertaining the strength of materials are, I believe, new, and if you think them worthy of a place in your Magazine, the insertion of them will oblige

Yours, &c.

T. T.

◆

Put f = the direct cohesive force of a square inch of the material;
b = the breadth;
d = the depth, or the dimension in the direction of the pressure; and,
l = the length.

Then the lateral or transverse strength of a rectangular prismatic beam or bar, is

$\frac{fbd^2}{6l}$ if supported at one end,

and $\frac{2fbd^2}{3l}$ if supported at both ends.

The lateral strength of a square beam or bar, when its diagonal is placed vertically, is

$\frac{fd^3}{24l}$ if supported at one end, and $\frac{fd^3}{6l}$ if supported at both ends.

In this case d is the diagonal. The strength of a square beam is the least when the force is in the direction of the diagonal. The lateral strength of a solid cylinder is

$\frac{fpr^3}{4l}$ if supported at one end,

and $\frac{fpr^3}{l}$ if supported at both ends.

In this case r is the radius, and $p = 3{\cdot}14159$ &c.

The lateral strength of a tube or hollow cylinder, is

$\frac{fp(R^4-r^4)}{4Rl}$ if supported at one end.

and $\frac{fp(R^4-r^4)}{Rl}$ if supported at both ends;

R and r being the radii of the exterior and interior circles. The section of the tube is supposed to retain its circular form at the time of fracture. It has been said that we cannot determine from theory the proper thickness of material for the tube, so that its form may not be sensibly altered by the pressure; but this is a mistake, for by a very simple investigation the thickness can be determined sufficiently near for any practical purpose.

The lateral strength of a triangular beam or bar is

$\frac{{\cdot}05643\, fbd^2}{l}$ if supported at one end,

and $\frac{{\cdot}22572\, fbd^2}{l}$ if supported at both ends.

In this case b is the base of the triangle, and d the height or the dimension in the direction of the pressure. The strength is the same when the edge is uppermost as it is when the opposite side is uppermost. The strength of a solid cylinder, pillar, or column, to resist a force acting in the direction of its axis is

$\frac{8fr^4}{5el^2}$, where e is the extension of the material at the time of fracture. The diameter of a column may be so great in proportion to its length, that a less force than that necessary to bend it, would crush it.

The force necessary to crush a homogeneous solid cylinder is $8fpr^2$.

The last rule has not yet been compared with experiment, indeed I do not know of any to compare it with. A set of experiments, on this kind of fracture, would be useful, not only to the architect and engineer, but also to the philosopher. "It is wonderful," says Professor Robison, "that in a matter of such unquestionable importance the public has not enabled some persons of judgement to make proper trials;" and we cannot without regret remark, that nothing worth notice has been added to our experimental knowledge since Robison wrote his excellent articles on the Strength of Materials.

If the rule given above is correct, the following table will show the weight that would crush cylinders of different kinds of materials.

Material.	Direct cohesion of a square inch.	Weight in pounds that will crush a cylinder an inch in diameter.	Weight in pounds that will crush a cylinder whose base is one foot in area.
	lbs.		
Cast iron . . .	50,000	314,160	57,600,000
Lead	3,000	18,849	3,456,000
Freestone . . .	1,000	6,283	1,152,000
Fine freestone	205	1,288	236,160
Good brick . .	280	1,759	322,560

VII. *Further Remarks on Dr.* Bradley's *Theorem for computing the Astronomic Refraction.* *By* T. S. Evans, *LL.D. F.L.S.*

The attention of astronomers has lately been much occupied with the astronomic refraction; and perhaps no other subject is so worthy their notice at this moment, since it is the greatest obstacle of any, to improvements and accuracy in that science. Much has lately been done towards an exact determination of its quantity at all altitudes above the horizon, by the use of the modern circles of Ramsden, Troughton, and other eminent instrument-makers. The great care with which these have been divided, and may be adjusted; the minuteness with which they may be read off, by means of micrometers, placed in the microscopes; and the certainty with which the object can be bisected, in the field of view, by using cobwebs, or perhaps the minute platina wires lately made by the ingenious Dr. Wollaston, give us great reason to hope, that with the zeal of our present best observers much time will not elapse, before the difficulties now met with in discovering the quantity of the refraction, will in a great measure, if not wholly, be removed.

Of all the different formulæ that have hitherto been published, it does not appear that any one of them at present will give the true refraction nearer than about six or seven seconds at ten degrees of altitude, and most of them reach to that extent with the same limits of error. It is therefore unnecessary to have recourse to the long and laborious methods that have been offered, when more simple ones will effect the purpose with equal exactness. Our attention should rather be directed to the improvement of some one of the number, that is easily computed so as to extend its application and bring it to correspond better in observations made at low altitudes. But if it should be found that the law of its progression does not admit of being expressed by a *formula*, we must endeavour to remedy the defect by means of a *table*, that will give its quantity at all necessary intervals.

According to the theory published by Mr. Thomas Simpson, in his Mathematical Dissertations, and further improved by Dr. Bradley, which, with modern determinations of the coefficients, is the one now most commonly used in England, on account of its simplicity, the equation for the astronomic refraction is

$$p \times \text{tang.}\left(Z.D - nr\right) \times \left(1 + \frac{\dot{\beta}}{\beta}\right) \times \left(\frac{1}{1+m\theta}\right)$$

which is evidently of the indeterminate kind, since it contains no less than four unknown quantities, that require to be discovered from other sources. These are p, n, r, and m; of which the first, p, is the refraction at 45° of altitude of the object above the horizon, taken at any given standard of temperature and density of the atmosphere. The second, n, is some multiple of the third, or mean refraction r, by which the zenith distance of the object is to be diminished before its tangent is taken out of the tables.

From a comparison of the theory with some accurate observations lately taken, I have had reason to think that n is not a *constant* multiplier of r, as has hitherto been supposed, but that it varies, according to some function of the altitude of the object above the horizon.

The fourth of these unknown coefficients, m, is the expansion of a volume of air, for each degree of ascent of Fahrenheit's thermometer; and the same comparison abovementioned, in conjunction with the latest and most exact experiments of two justly celebrated modern chemists has also furnished some strong reasons for doubting, whether the expansion for each degree of the thermometer is the same for all states of temperature, from the freezing to the boiling point.

This inquiry may, perhaps, by some persons be deemed interesting, as it points out a subject where the determinations of the chemist are corroborated by those of the astronomer.

These two branches of philosophy are apparently very distinct from each other, and separated by boundaries that do not at first sight appear even to approximate. It is therefore pleasing to discover a case wherein one of them is capable of affording assistance to the other; as it shows their mutual dependence, and consequently the advantages of cultivating and improving at the same time all the departments of human knowledge.

By consulting Dr. Thomson's valuable Treatise of Chemistry, vol. i. p. 494, on the expansion of atmospheric air, we read as follows:

"From the experiments of Dalton and Gay-Lussac it appears that all gaseous bodies whatever undergo the same expansion by the same addition of heat, supposing them placed under similar circumstances. Gay-Lussac found that air by being heated from 32° to 212°, expanded from 1000 to 1375 parts under a pressure $=0{\cdot}76$ of a metre ($=29{\cdot}92152$ English inches). Mr. Dalton found that 1000 parts of air, heated from 55° to 212°, expanded to 1325 parts. He found also, that the expansion of air is very nearly equable; or that the same increase of bulk takes place, by the same addition of caloric, at all different temperatures. The expansion from 55° to 133½°, was 167 parts; and from 133½ to 212 it was 158 parts. He has also shown that the expansion of air follows a regular geometric progression, if we suppose that mercury expands as the square of the temperature from the freezing point: and he has rendered it probable, that the expansion of water and mercury is as the square of the temperature of each, reckoning from their respective freezing points. He finds, if this law be supposed, that the expansion of water and mercury corresponds: hence he infers, that all liquids follow the same law; or that they expand as the square of the temperature, from the freezing point of each."

If we reduce these experiments to the same standard, and then bring their results into one point of view, they will be as follows:

First: M. Gay-Lussac found that air by being heated from 32° to 212° expanded from 1000 parts to 1375 parts. Therefore for 180 degrees it expanded $\frac{375}{1000}$ of the whole: consequently as $180 : \frac{375}{1000} :: 1 : \frac{375}{180000} = 0{\cdot}00208\dot{3}$ the expansion for each degree of the thermometer when taken throughout the whole extent of the scale from the freezing to the boiling point.

Secondly: Mr. Dalton found that air when heated from 55° to 212° expanded from 1000 to 1325 parts: therefore, as

157:

$157 : \frac{325}{1000} :: 1 : \frac{325}{157000} = 0{\cdot}002070064$ the expansion for that part of the scale reckoning from 55° to 212°

Thirdly: Mr. Dalton found that when the air was heated from 55° to 133½ only, it expanded from 1000 to 1167 parts; consequently as $78{\cdot}5 : \frac{167}{1000} :: 1 : \frac{167}{78500} = 0{\cdot}0021273885$ the expansion for each degree of the scale from 55° to 133½°.

Fourthly: The same ingenious experimentalist found that for an accession of heat from 133½ to 212° the increase of bulk was from 1000 to 1158 parts; we have therefore as $78{\cdot}5 : \frac{158}{1000} :: 1 : \frac{158}{78500} = 0{\cdot}00201273885$, the rate of expansion reaching from 133½ to 212: and by bringing these together for the purpose of more easy comparison, we find that

From 55° to 133½, the rate of expansion is	0·002127389
32 to 212	0·002083333
55 to 212	0·002070064
133½ to 212	0·002012739

The greatest rate of expansion in this statement is 0·002127389, and the temperature corresponding is lower than either of the others, viz. from 55 to 133½. The least rate is 0·002012739; and its corresponding temperature is from 133½ to 212, which is the highest of all the four. Of the other two, that from 32 to 212 includes a lower part of the scale than the one reaching from 55 to 212; and we find the rate of expansion is proportionally greater. It therefore appears, that the rate of expansion for the low temperatures is greater than that for the high ones; and consequently m, the coefficient of θ, in that part of the foregoing theorem which depends on the heat of the atmosphere, viz. $\frac{1}{1+m\theta}$ ought to be variable according to the different heights of the mercury in the thermometer.

Although the above table shows pretty clearly that the rate of expansion is greater in the low temperatures than in the high, yet there is not a sufficient number of them, nor are they made at the proper intervals to enable us to compute the exact law of this rate, for all degrees, from the freezing to the boiling point, which for the subject under consideration is very much to be desired.

These are the determinations of the chemists. Let us now endeavour to discover how far they are corroborated by the observations of the astronomer.

In the last communication which Mr. Groombridge has furnished us with, on the subject of refraction (Phil. Trans. for 1814), if we arrange the values of n, determined by that gentleman, according to the different states of the thermometer, omitting those that belong to very low altitudes as they are materially affected by other considerations they will be as follows:

Therm.	Values of *n*.
32°	3·74129
34	3·59582
36	3·70809
36	3·65335
37	3·63439
39	3·64778
39	3·21379 *
40	3·62402
40	3·62441
40	3·65160
42	3·64000
43	3·63706
43½	3·62255
53	3·63505
53	3·16032 *
54	3·35247
55	3·61333
57	3·49971
59	3·54668
60	3·54576
61	3·61064
62	3·55849

At 32° the value of *n* is	3·74129
32 to 40 omitting the last 39 ..	3·66345
From 32 to 40 including the last 39 ..	3·59922
40 to 43½	3·63327
53 to 60 omitting the second 53 ..	3 52945
53 to 60 including the second 53 ..	3·46793
60 to 62	3·57163

A slight examination of the above values of *n* will convince us that there is an evident change from 32° to 60°. Now if in the preceding formula we omit the part

$$p \times \left(1 + \frac{\beta'}{\beta}\right),$$

or consider it as being constant; and take only the remaining part of the expression, viz.

$$t,\left(Z.D - mr\right) \times \left(\frac{1}{1 + mt}\right),$$

or, $t, (ZD - nr) \times (1 - m\dot{\theta})$,

and substitute for n and for $\dot{\theta}$ their two extreme values taken from the foregoing table of means, we shall have in one instance,

$t, (ZD - 3{\cdot}74r) \times (1 - m \times 32)$,

and in the other,

$t, (ZD - 3{\cdot}47r) \times (1 - m \times 56)$, supposing the formula made for temperature zero.

Now in these two equations, the lower the mercury is in the thermometer, the greater is the value of n found to be: and the higher it is on the contrary, the less is the value of n: but to obtain the two values of n equal in the two equations, it is evident that the multiplier m must be increased at 32, and diminished at 56; which seems to be a further and interesting corroboration, of what was before stated, viz. that the rate of expansion for the low temperatures is greater than that for the high ones.

By carrying this mode of reasoning a little further, and supposing in the two last equations, that the values of $t, (ZD - nr)$ are constant, it would in that case be evident, that the variations of m would be inversely as the numbers 32 and 56; or that the rate of expansion would be in the inverse proportion of the height of the thermometer, provided the refraction and the rest of the coefficients remained the same. But as this is not the case, and the result thus obtained is far greater than is proved by experiments, and also that the ratio of the two values of m, in the two equations, is considerably nearer a ratio of equality, *it follows that the value of* n *must also vary*, and possibly, according to some function of ZD, or the zenith distance of the object.

The difficulty of determining the values of m, under all temperatures of the atmosphere, and of n at all altitudes, above the horizon will, from this, become evident: and it shows that they can only be obtained by some such method as that of the least squares proposed by M. Lagrange[*], and from a long series of observations taken at all periods of the year, in order to find them for the whole extent of the ranges of the barometer and thermometer.

Were all other circumstances to remain the same, and only the value of m to change, we might easily obtain it in the following manner:

The mean refraction r is to the apparent refraction ϱ as 1 to $\frac{1}{1 + m\dot{\theta}}$, or $r : \varrho :: 1 + m\dot{\theta} : 1$; and therefore $r = \varrho\,(1 + m\dot{\theta})$, consequently $m = \frac{r}{\varrho\dot{\theta}} - \frac{1}{\dot{\theta}}$; and by taking the different values of ϱ and $\dot{\theta}$, as found from the observations, we may obtain that of *m* for all heights of the mercury in the thermometer.

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[*] New Method of determining the Orbits of Comets. Paris, 1806. 4to.

With respect to the value of *p*, Mr. Delambre found it to be 60·499872″ at 45 degrees of apparent altitude, when the barometer stood at 29·92152 English inches, and Fahrenheit's thermometer at 32°. Mr. Groombridge found it 58·132967″ at the same altitude, when the barometer was 29 60 English inches, and the thermometer at 45°.

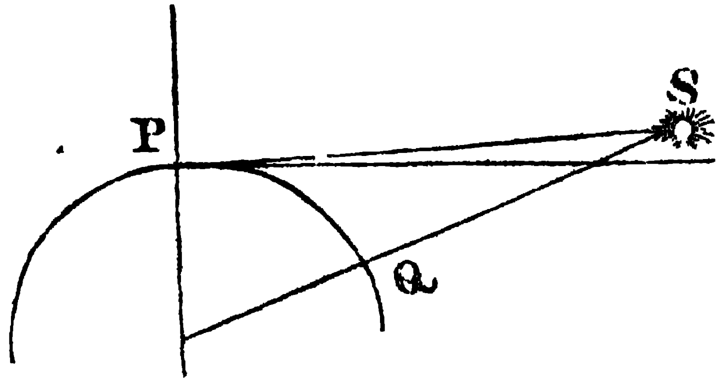
To compare these two values of *p* together, it is necessary to reduce one of them to the tenor of the other. Thus Mr. Delambre's determination, when brought to the same state of the atmosphere as that of Mr. Groombridge, will be

$$69{\cdot}499872'' \times \frac{29{\cdot}60}{29{\cdot}92152}\left(\frac{1}{1+(45-32)0{\cdot}002}\right):=58.33311''.$$

and Mr. Groombridge makes his 58·132967″; the difference 0·200143″ is small, and confirms the accuracy of the observations of both; yet small as it appears, it will very materially affect the refractions from 87° downwards to the horizon.

It is much to be doubted, whether this theorem or any other can be brought to agree with the utmost exactness in all cases with the refraction observed at very low altitudes, and for the following reasons:

The ray P S in its progress through the atmosphere from the



star S to the observer P, has probably to encounter layers of air of various temperatures and densities, which differ considerably from that indicated by the barometer and thermometer fixed up for use at P, the station of the observer; therefore, to be enabled to form any just conjecture of the deviations which the ray may have undergone, we ought to know the state of the atmosphere all the way from P to S; or at least we ought to be acquainted with the heat and density indicated by these two instruments along the surface of the earth, from the station of the observer at P to Q, the point immediately beneath the object observed.

It is well known that showers of rain often proceed in zones along the surface of the earth, in various directions; and should this have taken place across any part of the arc P Q, a short time previous to the observation being made, the evaporation that follows, by producing a greater degree of cold, will cause a considerable augmentation in the refractive power of the circumambient air.

A large surface of sand, situated any where along the arc P Q will, by absorbing a considerable portion of heat from the sun's rays, and afterwards continuing to restore it to the atmosphere, longer than the neighbouring soils that have absorbed less, by keeping up the rarefaction, bend the ray out of its course, in an opposite direction to that of the former cause.

A large wood or a marsh will also be the cause of deviation in the trajectory of the ray: and all these actions on its course will be the stronger the nearer it lies to the horizon, or in other words the less the altitude of the object.

Mr. Delambre, in the excellent Treatise of Astronomy which he has lately published, makes the following remarks on the refraction at low altitudes.

" It follows from all this that Simpson's Formula and Bradley's Table will not agree with the observations. There is an error of about 8″ at 82½°, and no known table will agree with them. That of the Board of Longitude computed from Laplace's formula, and on the value which I found for the constant quantity α, (the refraction at 45° which is p of our formula) agrees better, but the error is still from 2″ to 3″.

" I have already spoken of the uncertainty of observations of refraction in the neighbourhood of the horizon. I have remarked, from one day to the next, and in circumstances which were the same in appearance, that the refraction would vary from 15″ to 20″, without my being able to assign any cause; but the variations are still more sensible in the horizon, as we may judge from the following statement:

Computed zenith distance.	Observed zenith distance.	Refraction.	Barom.	Therm. of 80.
90° 44′ 5·4″	90° 8′ 36·8″	35′ 26·8′	27 6·0	16·64
90 33 39·2	90 2 43·6	30 55·6	27 6·0	16·64
90 33 9·1	90 2 12·7	30 57	27 7·4	20·64
90 33 13·0	90 1 53	31 19·6	27 6·5	20·32
90 27 50 6	89 54 36	33 14·6	27 8·1	11·84
90 39 34·5	90 4 37	34 57·4	27 6·3	19·20

" All these observations were made in June at sunrise. From the first to the second there was an interval of eight days, and of eleven from the second to the third. The barometer has scarcely varied at all, and the thermometer has varied but little, yet the refraction has varied 4 minutes at these zenith distances. According to our last tables the refraction changes from 11″ to 12″ for each minute of variation in the zenith distance. That of Bradley and of all the other astronomers varies from 10″ to 11″. Supposing 11″ of variation and reducing all these refractions to the astronomic horizon, that is to 90° of apparent zenith distance, we shall have for the horizontal refraction,

$$\left.\begin{array}{rr} 33' & 52'' \\ 30 & 33 \\ 30 & 33 \\ 31 & 6 \\ 34 & 15 \\ 34 & 12 \end{array}\right\} \text{of which the mean is } 32'\ 25''.$$

"From the first to the second day we have a difference of 3′ 19″, although the barometer and the thermometer are the same. From the second to the third, the refraction has not varied, although the thermometer has got up 4 degrees. The two last days the refraction has only altered 3″, although the thermometer has got up 7·36 degrees.

"We cannot therefore be certain of the mean to 2 minutes, which agrees nearly with that of Cassini. It is hardly probable that we shall ever be able to compute such anomalies; and what would they have been had I observed in winter?

"At 75″ I could not make the observations of different days agree nearer than about 6″ or 7″ between the extreme values.

"At 77° I have had variations of 10″ and 11″.

"At 79° they were 15″.

"At 82° they differed as much as 36″; that is, the table that I had constructed representing the observations of several days to 1″ or 2″ nearly, was found once in error −17″, and another time +19″.

"At 84° I have been more fortunate, the error was one half less.

"At 86° the difference between the extremes was 30″.

"At 88° the errors, which were nothing for several days, increased then to +15″ and −20″.

"At 89° from −15″ to +30″."

"The tables of Bradley and Mayer give still greater errors, so that it appears to me impossible to make any good table for these lower degrees. But from the zenith to 82°, we may have a number of tables nearly all equally good.

"In the fifth book of *Specola di Palermo* by Piazzi, you may find a great number of observed refractions. I have recomputed all the calculations, and have found them very accurate. Differences at least equal to mine, may be remarked among them.

"In the observations of zenith distances of terrestrial objects, I have repeatedly noticed, that at the setting of the sun the refraction increased from 2 minutes to 2½ minutes; so that objects which were hid during the whole of the day, became visible in the evening. (See *Base du Syst. Metr. Decimal*, t. i. pag. 157, 159, and 165). I have never seen that the state of the hygrometer had any sensible influence on the terrestrial refraction; (ibid. page 166). Messrs. Laplace, Gay-Lussac and Biot have proved, that it does not produce any change in the astronomic refraction."—*Delambre's Astr.* tom. i. p. 318.

It is therefore evident, that to complete the solution of this problem much yet remains to be done. From the present view of the subject, it would appear as if the best mode would be, to take successive altitudes of objects with an accurate altitude and azimuth instrument, from the times of their rising in the eastern part of the horizon, till they set in the western part, giving the preference to those that pass the meridian nearest to the horizon below the pole: and to repeat the observations at various seasons of the year under a great variety of densities and temperatures of the atmosphere. Each single observation should be recorded, and not the mean of a number, as is usual for other purposes in astronomy. They should be taken in as quick succession near the horizon as accuracy will admit; but higher up a smaller number will be sufficient. The meridional altitude should also be taken with great care above the pole when the state of the weather is favourable, as that will materially assist in the calculation.

By this means, the latitude of the place being known, together with the declination and azimuth of the object, or the time when the observation was made, we may, by computation, obtain its true altitude; and the difference between this and the observed altitude will be the refraction at that point. In this manner, from a number of observations, we shall obtain the refraction at every degree of altitude, from the horizon to the zenith; and taking them under different densities and temperatures, we may by equations of condition, or the method of the least squares, develop the whole, and assign, with the utmost accuracy, to each unknown quantity in the formula, its due magnitude in all its variety of circumstances.

The method is, it must be acknowledged, very laborious, and will require much time, both in making the observations and in computing them; but the advantages which astronomy will derive from an exact determination of this equation, are proportionally great. Did the duties of my situation afford sufficient leisure, and the smoke of London permit the observations to be made, I should feel the utmost gratification in performing this labour, tedious as it appears to be.

Thus I have endeavoured to show in what points this theorem is defective, not with any view to depreciate its value, but to assist in rendering it more perfect. It is the most simple one we have: and when the four unknown coefficients beforementioned, can be ascertained for all altitudes, densities and temperatures, a great obstacle to improvement will be removed, and the future progress of astronomy much facilitated.

Christ's Hospital, Nov. 15, 1815.

VIII. *On the Metallic Salts.*

To Mr. Tilloch.

Sir, — I beg leave to obtrude myself again on your notice, to offer a few remarks on the metallic salts, in reply to the answer of your correspondent H. (p. 246, vol. xlvi,) to my paper (p. 44 same volume). This I feel myself impelled to do, not only from the importance of the subject, but from some statements which H. has adduced as unobjectionable facts in support of the theory contained in his former paper. Before I proceed, however, it will be necessary to define the import of the words *free* and *excess* as hereafter used, philosophical discussions requiring a strict adherence to terms. Indeed an omission of this kind has produced some degree of displeasure in your correspondent, who objects to my using the words *free* and *excess* indiscriminately, which alone should be attributed to the different opinions we entertain of the subject. By *acid* in *excess* I mean, when the superabundance of acid is chemically combined with the base, and forms a constituent part of the salt. By *free acid*, when mechanically united or only adhering to the salt.

The inquiry is, if an excess of acid is really essential to the existence of all metallic salts. The arguments on one side tend to show that it is essential, because their solutions constantly indicate acidity with the test; on the other side it is maintained, that although free acid may, and in general appears to be necessary for solution, yet it is not required for the existence of the salt.

As I endeavoured to prove that solubility was increased by an increase of acid, H. in his answer draws the conclusion that, if such was the case, a salt would not be precipitated by acid in any proportion, and that the acid must be the solvent. And he alleges in opposition, that the greater proportion of free acid, rather than render a metallic salt more soluble, will in most instances precipitate it, and that the water alone was the solvent;

otherwise one substance would become alternately the solvent and precipitant of the salt.

From the tenor of our papers it cannot be misconceived that water was intended as the medium of solution, particularly as I stated the acid as the agent promoting the solubility, not as dissolving the salt; and those least acquainted with chemistry are fully satisfied that neither the salts, alkalies, nor even acids themselves are soluble without such medium.

H. evidently finds a difficulty in the attempt to reconcile the incongruity of one body assisting alternately the solution and precipitation of a salt. With him I should find some hesitation in admitting the fact, unless it could be accounted for on rational principles: but we observe ammonia precipitating and redissolving the metallic salts; we observe likewise water saturated with a salt beginning to crystallize,—the crystallization proceeding to a certain point, and the water then redissolving the crystals deposited. It is needless here to enter into the causes producing such effects, I merely wished to show that chemistry was not without facts of this kind. With respect to the case under consideration, acid cannot be added to an unlimited extent; for upon the addition of too considerable a quantity the salt may be deprived entirely of the water by superior affinity, as Berthollet has clearly demonstrated.

The slight degree of acid, either in excess or free, which is apparent by the test, unless supported by other very powerful proofs cannot be depended on as decisive: the test proving no more than that $\frac{1}{4000}$dth or $\frac{1}{3000}$dth part of the solution was acid, which minute portion might possibly adhere to the crystals.

If a sufficient quantity of acid existed in the salts to require the preposition *super*, an effervescence on the affusion of water and the addition of carbonated alkali, would be observed invariably; but which is seldom the case with the neutral salts carefully procured. Besides, many crystallized salts, on the addition of water, divide into super- and sub-salts; the former soluble, the latter insoluble or nearly so: and sometimes the attraction of the water for the acid is sufficiently strong in a degree to overcome the attraction of the acid for the base; either of which might be given as a reason for the indication of acidity.

The salts of lead are conspicuous as being generally insoluble, owing probably to the great attraction between the acid and base; and it has not been denied them, on that account, to class with the neutrals, and they are soluble on the addition of free acid. Not only an increase of the acid forming the salt, but often the addition of a different acid, as the acetic acid to the muriat of lead, the nitric acid to the sulphat of silver, &c., promotes materially the solubility.

In support of H.'s assertion, that metallic salts are super-salts with excess of oxide, he instances the formation of sulphat of iron; and says, that after the action ceases, if sufficient iron had been introduced, part of it remains uncombined, and the solution acid; but this arises, I conceive, from the want of contact between the free acid and iron, the crystals as they form surrounding it; and this, perhaps, is one reason why sulphat of iron is seldom obtained unmixed with the super-sulphat.

He states also that acid is indicated on the application of the test to a large crystal of sulphat of copper on breaking it. I never had the satisfaction of obtaining a large crystal of the salt, and am inclined to suspect it more than probable he might try the blue vitriol of commerce, which is well known to be a super-salt, the neutral sulphat being seldom, if ever, obtained; or, if he really tried a large crystal of the neutral sulphat, it is not unlikely that a minute portion of acid might have been retained in the interstices or between the laminæ of the crystal.

My opinion is, that there can be no solution of a metallic salt without acid either free or in excess. Fourcroy, as before observed, noticed this, as well as Bergman and most other chemical writers; and as this superabundance always appeared on the solution of the salt, or was required to be added to enable the salt to dissolve, it is natural to conclude that it is essential either to the existence or to the solution of the salt. I am inclined to adopt the latter opinion.

Were the reasons before stated insufficient, I think the doctrine of Dalton decisive in favour of the existing theory. From the mean of the most correct analysis, it appears that the binary and ternary salts are composed of an equal number of atoms of base and acid, or the one a multiple of the other, whether alkaline, earthy, or metallic; and that, with very few exceptions, the neutral salts contain an equal number of atoms of base and acid, and in the super- and sub-salts the acid or base is a multiple of the other respectively; a coincidence so strong, and in my judgement so conclusive, that if a minute quantity of acid can be detected in a neutral salt, it can only be considered in a *free* state, and by no means a constituent part of the salt.

I am, sir,
Most obediently yours,

Stoke Newington, Jan. 1, 1816.
G. S.

IX. *On the dispersive Power of the Atmosphere; and its Effect on astronomical Observations. By* STEPHEN LEE, *Clerk and Librarian to the Royal Society. Communicated by* W. H. WOLLASTON, *M.D. Sec. R.S.*[*]

NOTWITHSTANDING the pains which astronomers have taken to determine accurately the refraction of mixed light, nothing, I believe, has ever been done towards ascertaining the dispersive power of common air, or comparative degree of refrangibility of the differently coloured rays in their passage through our atmosphere.

The importance of such an inquiry, however, must be obvious to every one who duly considers the effect which the different degrees of refrangibility of the variously coloured lights must necessarily produce in the apparent situations of differently-coloured objects. Stars of different colours must be differently refracted, and the apparent altitude of the sun must vary according to the colour of the dark glass through which he is viewed.

Perhaps this cause alone is sufficient to explain the disagreement which is found to exist between the latitude of a place deduced from observations of circumpolar stars, and that deduced from observations of the sun during the solstices, which has so long occupied the attention of astronomers, and has never yet been satisfactorily accounted for[†].

The dispersive power of the atmosphere will also show why Aldebaran and the red stars are sometimes seen projected on the moon's disk in occultations by that planet, especially when the immersion or emersion happens to be near her upper limb. For the light of the moon being white, is more refracted than that of the star, and consequently her limb more elevated, which would occasion the star to appear within her disk a few seconds before or after contact[‡].

The great disagreement which is found to exist in the declination of several of the fixed stars, as given by different observers, may probably be traced to the same cause, stars being more or less refracted according to the predominant colour of which their light is composed.

That the fixed stars differ from each other in respect to the composition of their light, must be obvious to any one who will only take the trouble of comparing them on a fine night. They present a striking variety of colour even to the naked eye. But this difference becomes still more perceptible when they are viewed through a prism properly adapted to the eye-piece of a reflecting telescope.

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[*] From the Philosophical Transactions for 1815, part ii.

[†] Vide Mr. Piazzi's Memoir on the Obliquity of the Ecliptic, in the Memoirs of the *Società Italiana*, vol. xi.

[‡] Vide Philosophical Transactions, vol. lxxxiv. p. 345. *Histoire Céleste Françoise*, tom. i. p. 393, 403, 418, 425, 428, 467, and *Connoissance des Temps* for 1817.

A star viewed in this manner is converted into a prismatic spectrum. Sirius and the *brilliant* white stars exhibit a large brush of beautiful violet, and the most refrangible colours in great abundance. Aldebaran, α Orionis, and the red stars show only a small proportion of those colours, whilst the dull white stars exhibit a great quantity of intense green light.

The planets also differ much from each other in this respect. The moon, Venus, and Jupiter, seem to possess every colour; but the green is very pale in all of them. Mercury and Mars appear deficient in the middle and most refrangible rays, whilst the light of Saturn seems to be composed principally of the mean rays with a very small proportion of the extreme colours of the prism *.

The different refrangibility of the differently coloured rays is very visible in stars near the horizon. If viewed on a fine night with a power of 200 and upwards, they appear expanded into a prismatic spectrum. Sirius, when within a few degrees of the horizon, presents a most beautiful object.

Having remarked the very oblong figure which the spectrum assumes when near the horizon, and found from repeated observations of different stars that the separation of light begins to be visible as high as 40° or 50° of altitude, I was led to believe that the dispersive power of the atmosphere must be sufficient, in many cases, to produce considerable effect on astronomical observations; and, consequently, to suppose that it would be desirable to ascertain, if possible, the exact degree of separation of the several rays †.

With this view, therefore, I began a series of observations; the result of which, and the manner of conducting them, I shall now take the liberty of laying before the Society.

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* Query. May not this circumstance explain why Saturn, though less brilliant, bears magnifying better than Jupiter and Venus!

† Dr. Herschel, in a note to his paper on Double Stars, published in the seventy-fifth volume of the Philosophical Transactions, says that the prismatic power of the atmosphere is very visible in low stars; and very justly observes that this power ought not to be overlooked in delicate and low observations: he gives the measure of two diameters of ε Sagittarii, which seem to indicate that the refraction of the extreme rays is about $\frac{1}{74}$ ±, the mean refraction. I think it due to that great astronomer to mention the circumstance, though it was totally unknown to me till long after I had completed my observations on Mars

The first instrument employed for the purpose with any degree of satisfaction, was the two-feet reflector made by Mr. Short, and which belongs to the Royal Society. In the compound focus of the eye-piece of this telescope, I fixed *horizontally* a narrow slip of ivory. With the instrument thus prepared, I observed Capella, and other low stars near the meridian. By carefully noticing the intervals of time between the first contact and total immersion, and between the first appearance and complete emersion of the star from behind the slip of ivory, I obtained data from which it was easy to calculate its vertical breadth, which, compared by estimation with its horizontal breadth, gave the separation of the extreme rays of light.

It was impossible, however, to remain long satisfied with such coarse measures, and not finding it convenient to go to much expense on this account, I applied to my friend Mr. Rennie for the loan of his seven-feet reflector made by Dr. Herschel, to which I adapted a very excellent wire micrometer made by Mr. Troughton; and thus, by the kind assistance of my friends, I obtained instruments capable of measuring small angles to the fraction of a second of space.

With this apparatus I repeatedly measured the diameter of Mars during his opposition in 1813. The Society's apartments being well situated for the purpose, I observed the planet as soon as he became visible over the buildings, until he attained his meridian altitude, which never exceeded 15°.

With a power of 170 and upwards, the disk of the planet appeared much elongated, especially when near the horizon; the upper limb was of a fine blue, the lower limb of a deep red.

By carefully measuring the diameter of Mars and the breadth of the coloured edges, I endeavoured to ascertain, as exactly as possible, the degree of separation of the differently coloured images of the planet.

But after all it was no easy matter to measure the coloured edges exactly, for the light which was necessary to illuminate the wires, rendered the colours so very faint as to make it extremely difficult to distinguish their precise boundaries. For this reason, and because I wished to apply higher powers than could be used with the micrometer, I adopted the following method, which I found far more convenient, and is, I believe, quite as accurate.

I drew on a sheet of paper several figures of two equal circles cutting each other, placing the centres of the circles in the first figure (Plate I.) $\frac{1}{10}$th of their radius from each other; in the second figure $\frac{2}{10}$ths; in the third $\frac{3}{10}$ths; and so on. The upper crescent of these figures I painted blue, the lower crescent red, and the part common to both circles of a reddish yellow, softening the colours into each other as they appeared in the planet. For I considered that, in fact, it was not a single image of Mars that was seen, but a number of differently coloured images, lying in the same direction, though lifted one above another, as represented in the annexed figures.

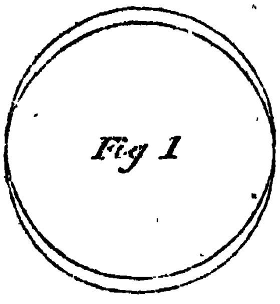
Fig 1

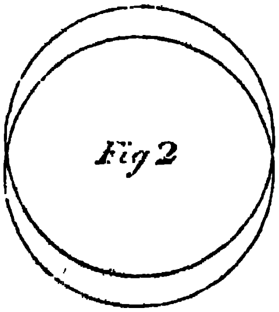
Fig 2

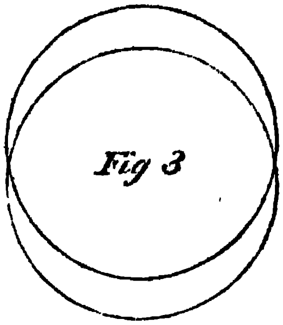
Fig 3

Having prepared a number of these drawings, I repeatedly compared them with the planet viewed through the telescope with different magnifying powers, carefully noting which figure he most resembled, and the time of observation.

This being done, it was easy to calculate the exact altitude from the time of observation, and to make a very near estimate of the separation of the images from the figure referred to, compared with the diameter of the planet found by the micrometer.

From a great number of observations on Mars, Venus, and the fixed stars, taken in all these different ways, I found the deviation of the extreme rays of light to be between $\frac{1}{60}$th and $\frac{1}{70}$th part of the total refraction.

It has already been observed, that the disagreement which is found to exist between the latitude of a place deduced from observations of circumpolar stars, and that from observations of the sun, may perhaps be traced to the use of dark glasses. But this will appear more evidently from a reference to the method employed by Dr. Bradley for determining the quantity of refraction, which method is very clearly described by Dr. Maskelyne in the seventy-seventh volume of the Philosophical Transactions. He says,

That Dr. Bradley got the height of the pole from observations of the circumpolar stars, and the height of the equator from observations of the sun at the two equinoxes; that he found these two altitudes together amounted to 89°, 58′, 3″, which being subtracted from 90°, leaves 1′, 57″, for the sum of the refractions at the pole and equator; and that of this quantity he assigned 45½″ to the former, and 71¼″ to the latter.

But Dr. Bradley undoubtedly made use of dark glasses for observing the sun, probably smoked glasses, which would give him a pale orange-coloured image, or one of less than mean refrangibility; consequently, the quantity of refraction as found by Dr. Bradley must be too small for white light.

This alone is sufficient to produce a small difference between the results of our observations of the sun and of the stars. I shall now mention two other circumstances which appear to me to have produced a still greater apparent disagreement.

The publication of the Nautical Almanack in 1767, led to the general use of Hadley's sextant. In the construction of this instrument, coloured glasses were indispensibly necessary; and the great convenience in the use of them over smoked glasses, soon occasioned the application of them to all other instruments. These glasses generally give a deep-red image, or one of less refrangibility, than smoked glass. The effect of this alteration, therefore, should have been, that arising from too great correction for refraction in every thing depending on observations of the sun.

The introduction of achromatic object-glasses* produced an error of a different kind; and one which, in certain cases, tends to correct the other. In the single object-glass telescope (and there were no others in Bradley's time) the differently coloured images are formed at different focal distances, which, in a manner, compels the observer to adjust his instrument to the most intense light; that is to say, to the orange-coloured † image; by this means the fainter colours, which occupy the greatest space in the spectrum ‡, are dissipated, and lost among the more powerful rays. In good achromatic telescopes the case is very different, for all the rays being collected by them into one point, every colour is seen in its proper place; so that the observer, in bisecting the spectrum, takes the altitude of the mean, or the upper extremity of the green image.

But if the upper extremity of the green image be taken in observations of circumpolar stars, a greater correction than Dr. Bradley's ought to be applied, in order to get the true height of the pole.

It may not be amiss to observe here, that the observations of Mr. Lalande at Paris, show a greater disagreement than those at Greenwich; and the observations of Mr. Piazzi at Palermo, a still greater than those of Mr. Lalande. This, I apprehend, must arise partly from the lesser elevation of the pole in those places, and partly from the fainter colours in the stellar spectra being more distinctly visible in the clear atmospheres of France and Italy than in England.

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* An achromatic object-glass was first applied to the south quadrant at Greenwich in 1772, and to the north quadrant in 1789.

† Vide Newton's Optics, book I. part i. prop. vii.

‡ Ibid. book I. part ii. prop. iii.

It should seem then, that in order to get a perfect knowledge of astronomical refraction, we ought to employ at least three different methods of investigation. 1st. By observations of the fixed stars during the night, when all the prismatic colours are visible. 2dly. By observations of the stars during the day, when none but the orange-coloured rays are to be seen. And 3dly. By observations of the sun with different coloured glasses. By these means we might hope to obtain such an accurate knowledge of atmospheric refraction as would enable us to form tables adapted to every possible circumstance.

But I must not take up the time of the Society by any additional observations. It is in vain for me to pursue the subject any further, in a situation so ill-adapted to astronomical observations as Somerset Place: I shall therefore resign the task to those who are more favourably placed in this respect, and who possess instruments better calculated for an investigation which requires so much accuracy.

X. *A Geological Sketch of a Part of Cumberland and Westmoreland. By* A CORRESPONDENT.

THE geology of Great Britain has of late years become an object of considerable attention, and much has been done in acquiring a knowledge of the different rocks, their relative position, and the extent they occupy on the surface,—more especially in the south of England, where the different formations appear to have been described with as much accuracy as the present state of our knowledge of the subject will admit. In the northern counties, however, little has been done except so far as regards the Newcastle coal-formation, and the metalliferous rocks of the Cross Fell range of mountains; and although the counties of Cumberland and Westmoreland have been frequently visited by very eminent mineralogists, and some attempts have been made in describing their geology; yet from the multiplicity of formations they contain, these formations occurring in such detached and insulated patches, and the very great difficulty in investigating and ascertaining their relative positions, we must consider our knowledge of the physical structure of these counties as still very imperfect. As this knowledge can only be acquired by the united efforts of a number of individuals, I take the liberty of offering to this Society*, the following notices of some rocks which seem hitherto to have escaped the attention of mineralogists. The Cross Fell range of mountains is composed of limestone, sandstone, slate-clay, coal and greenstone; the dip of the strata is towards the south-east, the mountains are very precipitous towards the west, and generally rest upon the old red sandstone; but that this is not always the case, the following facts will prove. At the foot of the Cross Fell range towards the west, a series of small conical hills make their appearance, which I have traced from beside Helton near Brough in Westmoreland, to the boundary of the county of Cumberland, a distance of about twelve miles; but the base of these hills is no where more than three quarters of a mile in breadth. From the observations of Professor Buckland, of which a short notice was published in Thomson's Annals, it appears that these hills, or at least the rocks which compose them, extend considerably further north, at least as far as Melmerby in Cumberland; but this was unknown to me at the time I examined this neighbourhood. These hills or pikes, as they are called in the county, are composed of greenstone, grau-wacke and grau-wacke slate; and on the north side of Dufton pike, which is the central one, granite is found.

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* The Literary and Philosophical Society, Newcastle.

These primitive and transition rocks do not form a continued line, but are interrupted by the red sandstone which nearly surrounds the hills or pikes, till it almost comes in contact with the limestone rocks: but although I have seen these rocks on the surface at the distance of not more than 50 yards from each other, I have never been able to find their junction, nor to satisfy myself as to their relative positions. At Dufton there is a very good section of the junction of the metalliferous rocks with the grau-wacke. The limestone, &c. in approaching the older rocks, vary from their nearly horizontal position, and rise towards the west at an angle of about 45° against the side of Dufton pike; and on the other side of the pike we find the red sandstone dipping towards the west. On Dufton pike both the granite and grau-wacke have been quarried for the purpose of repairing the roads.

At Warcop the grau-wacke is covered by a very singular limestone, some strata of which are so full of fragments of quartz, that at first sight I took it for an alternation of grau-wacke with limestone. The other strata are, however, compact, resembling basalt with small spangles of mica interspersed, and there is one of oolite; the dip of this limestone is to the west, at an angle of 10° or 12°, and its outgoings are covered by the metalliferous rocks, which, as is always the case in this neighbourhood upon their coming near the older rocks, have their inclination increased; but the increase here is very great, making an angle of not less than 70° with the horizon. This limestone is not more than half a mile in breadth, when the red sandstone is found in nearly a conformable position, and appears to rest upon the limestone; but of this I am not certain, as I could not see these rocks in contact. I am inclined to consider this limestone as belonging to the transition class, although I am aware that it will be objected to, on account of its having beds of oolite, which is always considered as belonging to the newest flœtz. I am afraid we fetter ourselves too much by considering certain fossils as belonging exclusively to certain classes of rocks, by which means many encrusting fossils are overlooked. Mr. Bigg found asbestus in greenstone at Melmerby Scar, and I have lately found the same substance forming veins in transition greenstone in the neighbourhood of Keswick:—now this mineral is always described as belonging only to serpentine rocks. I found carbonate of strontian in the basalt of the Giant's Causeway, and this substance is said only to belong to primitive mountains.

It is now about twelve years ago since I first observed granite on Skiddaw and Saddleback, since when it has been frequently visited; but its relation with the other rocks composing these mountains not having been ascertained, I was induced to examine them this summer. The granite is found in the bed of the rivulet which separates these mountains, and extends northward across the river Calduo to Carrick Fell; it is rather small-grained, composed of white felspar, quartz, and grayish mica. The summits of Skiddaw and Saddleback are clay-slate and hornblende-slate in alternate beds, dipping to the south at an angle of about 60°, and resting immediately on the granite; but on Carrick Fell this rock is covered by gneiss, the felspar of which is of a grayish-white colour; the mica abundant, but in minute scales; so that this rock might be mistaken for a thin slaty micaceous sandstone, were it not sufficiently characterized by the felspar. The gneiss is again covered by mica-slate, which becoming mixed with hornblende, passes into hornblende-slate, thus proving the hornblende-slate, and consequently the clay-slate, to be of the same formation as the mica-slate. On the other part of Carrick Fell, the mica-slate is covered by sienite, which forms nearly the whole of the mountains of Carrick and Caldbeck Fells. This sienite varies very much in its appearance: the first rock of it we meet with is very small grained, consisting of reddish felspar-quartz, and very little green hornblende; after that a beautiful large-grained variety is found; the felspar white with black hornblende, the crystals of which are frequently nearly an inch in length. As we proceed northward the hornblende increases in quantity, and the sienite passes by almost imperceptible gradations into a compact greenish-black greenstone; and on the summit of High-pike this greenstone forms the basis of an elegant porphyry with crystals of green felspar resembling the antique porphyry. Near High-pike a strong east and west vein is wrought for lead; but, from the hardness of the rock and the difficulty of separating the numerous ores, I am afraid not to much advantage. The vein stones in this mine are quartz and sulphate of barytes, often so intemately mixed as to form a very singular stone. The ores are sulphuret of lead, carbonate of lead, and phosphate of lead *, blende, copper-pyrites, grey copper ore, green and blue copper ore, and malachite. The galena is antimonial, and is very rich in silver, yielding in the refining furnace about 60 ounces in the ton. I traced this vein westward for about three miles to a place called Silver Gill, where there is a very fine section of it on the surface; it is about three fathoms wide and hades to the north at an angle of about 65°. There is a tradition in the county, that at this place a rich silver mine was wrought in the time of queen Elizabeth, and certain it is that there are remains of very extensive workings. An attempt has been made last spring to recover this mine, for which purpose a trench has been cut across the direction of the vein so as to bore the rock; and although this trench is not more than 200 yards from the old works, the vein has not been found. The cause of failure is, that the sienite is here covered by grau-wacke slate, into which the vein does not penetrate: this is different from what has been observed in Cornwall, where the veins traverse both the granite and grau-wacke slate; and I consider it as an interesting fact, since it so decidedly marks the difference between these rocks. The clay-slate and hornblende slate on the summits of Skiddaw and Saddleback, with the granite gneiss and mica-slate, and the sienite of Carrick and Caldbeck Fells, must therefore belong to the primitive class of rocks; whilst the grau-wacke and grau-wacke slate which entirely surrounds these mountains are transition rocks. Some other veins have been wrought for copper in the sienite, but not to any great extent; nor do they appear to be of much value. Near Hatfield Hall is a vein containing the oxides of titanium and iron (the titan-eisan of the Germans);—By the bye, Why is this fossil not described in any mineralogical work?

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* I found one specimen which I believe to be muriate of lead; it has the following characters:—Crystal, a rectangular prism; colour, grayish-white; lustre, highly resplendent adamantine, much superior to carbonate of lead; hardness superior to carbonat of lead. Not wishing to injure the specimen, I am not able to give any other of its characters.

Mr. Sheffield discovered molybdina in the same neighbourhood; and I found a very well defined specimen of wolfram on quartz in the bed of the river Calduo.

The coal-formation of Whitehaven is covered at St. Bees Head by a curious bituminous clay, formed, as it were, of a number of hollow cones inserted into each other, till a column is formed whose length (about a foot) is equal to the thickness of the stratum, and by the lateral aggregation of these columns the whole bed is formed: over this is a limestone containing magnesia and iron, and exactly resembling some of the dark-coloured more compact beds at Whitby. Above this is a red sandstone, connected with which is clay, marl, and gypsum. Thus a beautiful uniformity exists on the two sides of the island; for if we suppose the limestone, &c. of the neighbourhood of Cockermouth to accord with the metalliferous rocks of Alston Moor, the Whitehaven coal-formation with that of Newcastle, the magnesian limestone of St. Bees with the Sunderland limestone, and the red sandstone with that of the Tees,—we have the series complete. Notwithstanding the opinion of Professor Kidd, that our rocks are in a state of absolute confusion, I think I could show that our rocks, though numerous and occurring in small compass, are remarkably regular in their relations: but it would be leading me too far from the object of this paper, which is merely intended as a notice of some rocks which have lately fallen under my observation.

XI. *Specimen of a new Nomenclature for Meteorological Science.* By Thomas Forster, *Esq.*

To Mr. Tilloch.

Sir,—The propensity which the English writers have to borrow from other languages terms used technically in the sciences, renders their works so very difficult to be understood by those who do not understand the languages from which the words are derived. Besides which, their habit of borrowing words has a tendency to obliterate the distinctive characters of different tongues, and has already made the English a motley and unphilosophical group of exotic words, which one might well suppose had sprung from the confusion of Babel. It is my intention to construct a scientific nomenclature (entirely out of our own language, or its mother-tongue the Saxon), and to adopt it in my journals of meteorology. I subjoin at present only a few technical names, which I shall henceforward use, and which I have already made, partly from the consideration above alluded to, and partly from having been repeatedly desired by artists and others who have made use of Mr. Howard's paper on the Clouds, and of my last Essay on Atmospheric Phænomena, in order to depict more accurately clouds, &c. and to make an English nomenclature, as many persons unaccustomed to Latin could not retain the distinctions of the Latin nomenclatures in their memories. The Latin terms for clouds, originally made by L. Howard, are very useful in descriptions in Latin, and in those which are to go abroad, or which are for scientific persons only; and should, I think, be inserted with the others. But we ought to use terms of science constructed out of the language in which that science is treated of.—I subjoin the following names for clouds, substituted for the former ones, and others substituted for those which I constructed from the Latin for *halos* and other phænomena made by refraction; and shall proceed in future to a more extended nomenclature for other terms of meteorological science.

CIRRUS, or CURLCLOUD. I propose to call this the curlcloud, from its constant tendency to assume the fibrous and flexible forms. It is bent and curled in all directions; and curlcloud comes nearest to its old name *cirrus*, of which *cirrulus* and *curl* are diminutives.

CIRROCUMULUS, or SONDERCLOUD, *i.e.* cloud consisting of an aggregate of clouds *asunder* (from A. S. *sond*, Old Eng. *asonder* and *sonder*): the distinguishing marks of this cloud being that of separate orbs aggregated together, and the change to this cloud from others is a separation of continuity into particules.

CIRROSTRATUS, or WANECLOUD. The constantly evanescent state of this cloud in all its forms suggests this name. It is always subsiding or altering its form, or *waning*, a verb now not so much used as formerly, and which comes from the Anglo-Saxon Fẏnıᵹean, *evanescere;* whence also our words *to faint*, *feint*, &c.

CUMULUS, or STACKENCLOUD, *i.e.* stackt cloud, from being piled or heaped up; *to stack* being a known verb for piling up. The participial termination *en* gives the word a pleasanter sound then *stackcloud*.

STRATUS, or FALLCLOUD, being the subsidence or falling of the aqueous particles in the evening, I first thought of *evencloud*, corresponding to the German *abendwolke;* but that is not definite enough, as many clouds become evening clouds.

CUMULOSTRATUS, or TWAINCLOUD; being the visible result often of the coalescence of two modifications (supposed with opposite electricities) and when it forms primarily a similar union being conjectured of particules separately electrified as soon as formed.

NIMBUS, or RAINCLOUD, may be subdivided, and described shortly, as *stormcloud*, *thundercloud*, &c.

HALOS will be called MOONRINGS, SUNRINGS, &c.

CORONAS will be called MOONCROWNS, &c. But I give this merely as an imperfect specimen of the nomenclature I am making, and which I shall publish and give in your Magazine in a short time: meanwhile I shall adopt these words of Saxon derivation in my journal, and give an explanation in the notes to those not inserted here.

I am, &c.

THOMAS FORSTER.

XII. *On the Influence of the Atmosphere on the Electro-Galvanic Column of M.* DE LUC. *By Mr.* J. TATUM.

To Mr. Tilloch.

SIR, — ALTHOUGH various accounts of Mr. De Luc's electrical column have appeared in your Magazine, I do not recollect that any journal of its action as connected with atmospherical influence has yet appeared. Having long observed the very considerable alteration which every thermometrical change in the atmosphere produced on the operation of this instrument, I determined on keeping a regular journal, and noticing its corresponding change with those indicated by the barometer, thermometer, De Luc's hygrometer, and the direction of the wind. The results of my observations as deduced from my daily register of those instruments have convinced me that the greatest changes in the oscillations of the ball on De Luc's columns are chiefly, if not entirely owing to the increased temperature of the atmosphere, and not to its moisture, as some of my philosophical friends had supposed. By the following extract of my journal, it appears that when the thermometer was 52° and hygrometer 81°, the ball of the columns oscillated only 248 times in a minute; but when the thermometer was 56° and the hygrometer 50°, it oscillated 284 times; a satisfactory proof that the heat and not the moisture of the atmosphere increased the action of the instrument. In the latter case, I had removed the instrument to the gallery of my lecture-room, where the thermometer was 14° higher, and the hygrometer 5° drier than at the place from whence it was removed.

It is proper also to add, that from the 1st till the 13th of the month the thermometer and hygrometer were not in the same apartment with the electrical columns, a circumstance which may have occasioned some difference in the results. Since that period the whole apparatus has been inclosed together in a glass case, where all experience the same atmospheric influence.

As my columns are constructed similar to those already described in your Magazine, it will be unnecessary to describe them any further than to say that the diameter of the balls is 2·65 inches, their distance from each other 0·58, and the diameter of the gold ball 0·4 inches. I have constructed several other columns of different diameters; and from a comparison of one which is only 0·53 of an inch in diameter with one which is 0·76 of an inch (and by which this register is kept), I am inclined to believe that the power of the instrument is very little increased by the diameter of the plates, but that it regularly increases with their number. But, as I am engaged in making other columns of different construction and diameter, I shall soon be enabled to speak more decisively on this head.

Date.	Direction of Wind.	Thermometer.	Barometer.	Hygrometer.	Oscillations in a minute.
Dec. 1	S	55	29·00	80	268
2	S	52	30·16	81	248
3	SE	51	30·10	79	240
4	NW	51	29·85	69	222
5	SW	44	30·0	68	210
6	NW	45	29·5	66	200
7	NW	43	29·85	71	200
8	N	31	30·05	60	186
9	N	29	30·10	61	168
10	N	34	30·40	65	182
11	N	36	30·40	63	188
12	N	44	30·35	63	194
13	SW	43	30·30	65	194
14	SW	45	30·35	59	192
15	SW	47	29·90	61	206
16	W	50	29·20	60	224
17	W	45	29·10	55	200
18	W	42	29·46	55	186
In the Gallery of Lecture-Room.		56	29·50	50	284
19	W	42	29·60	54	186
20	SW	45	29·40	56	200
21	SW	48	29·40	56	200

I am, sir,

Yours,

J. TATUM.

Dorset-street, Salisbury-square,
Dec. 21, 1815.

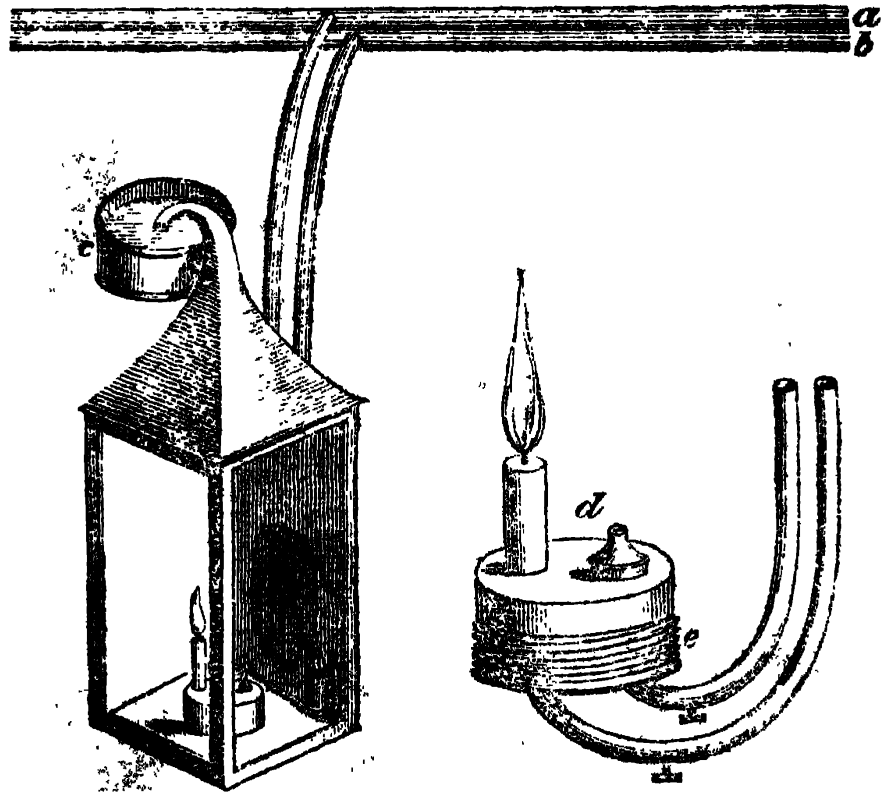
XIII. *De-*

XIII. *Description of a Gas Lamp for Coal-Mines, invented by Mr.* E. Carter, *of Exeter. Communicated in a Letter from Mr.* Carter *to Sir* H. Davy*.

Exeter, Dec. 27, 1815.

Sir, — The many shocking accounts of accidents in coal-mines from the explosion of gas, induced me (no doubt with many others) to consider some means of preventing such dreadful accidents. The contrivance of a lantern that might be kept as it were insulated from the atmosphere of the mine, appeared the most likely mode of preventing explosion; and I thought mine, of which I have subjoined a sketch, would fully answer the purpose. When I was about sending this to the Editor of The Philosophical Magazine, your experiments and very learned observations on the subject, and very ingenious contrivance of a safety-lantern, appeared in that work; the excellence of which in simplicity and portability rendered it unnecessary, I thought, for me to say any thing respecting mine. However, on reconsidering the matter, I am induced to think that the coal-mines may be lighted with much advantage, in point of expense, with gas, as the material for its production rises on the spot. My first plan was intended for burning oil, and the addition of the pipe for conducting gas is the only alteration made.

The pipes *a* and *b* are main pipes, the one to conduct gas from the gasometer, the other to bring a supply of air to maintain



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* Communicated by Sir H. Davy to the Editor.

the combustion: the air-pipe to be kept full by the degree of pressure that may be found requisite from bellows kept in motion by the steam-engine; the size of the bellows to be proportioned to the number of lights that are to be fed. The lantern should be made perfectly air-tight, the crown of which should terminate, as represented in the sketch, by a tube immersed in water contained in the basin *c*. The air vitiated by combustion would run into the tube and be forced through the water by the pressure of air from the bellows. On one side of the lantern hangs an air-tight glove fastened round a hole sufficiently large to admit a man's hand; and on the same side is a case of matches and a bottle which may be ignited without air escaping from the lantern, or finding its way into the lantern from the mine. The aperture for the hand may be further secured by a door on the outside. *d* is an enlarged sketch of the gas-burner and air-pipe which may be united with the lantern by the screw *e*.

Should this contrivance be founded on erroneous principles, which a very limited knowledge of science makes me doubtful of, I hope that I shall be forgiven this intrusion, and that an attempt to be useful will, with Sir Humphry Davy, be sufficient apology for any liberty of this nature.

I am, sir,
Your most obedient humble servant,
E. CARTER.

To Sir H. Davy, &c. &c. &c.

⁂ The idea of the "air-tight glove," suggested by Mr. Carter, deserves the attention of the mine-owners, as it furnishes a mean for relighting the lamp when extinguished, without any danger of an explosion.—*Note by Sir H. Davy.*

XIV. *Notices respecting New Books.*

On the Fire-damp of Coal-mines, from the Philosophical Transactions of the Royal Society. With an Advertisement containing an Account of an Invention for lighting the Mines and consuming the Fire-damp without Danger to the Miner. By Sir HUMPHRY DAVY, *LL.D. F.R.S. P.R.I.* 8vo.

SIR H. DAVY has published in an octavo form, the paper read before the Royal Society, inserted in our last number. The publication is prefaced with the following advertisement, which deserves to be very generally made known, as it describes a discovery made by Sir Humphry, since the reading of his paper, which may be considered as an invention even more important than that forming the subject of the paper itself.

This publication has the following dedication:

"To the Lord Bishop of Durham, who has taken that warm interest in the subject of these pages which might have been expected from his exalted benevolence.

"They are dedicated as a testimony of the respect and attachment of the author.

"ADVERTISEMENT.

"It is impossible to converse with persons in the neighbourhood of the collieries where explosions have happened from the fire-damp, and not to be strongly affected by the accounts they give of the destruction of human life and the variety of human misery produced by these dreadful accidents. By a single explosion in the Felling colliery* 94 persons were destroyed, and nearly as many families plunged into deep distress; and the frequency of the occurrence of these catastrophes, notwithstanding the improvements in the ventilation of the mines, and the continued activity of the persons concerned in the care of the works, had almost produced a feeling of despair in the minds of many benevolent persons, as to the possibility of finding a remedy sufficiently simple and œconomical to be used in the mines; and when I first turned my attention to the inquiry, it was rather with a faint hope than a strong expectation of discovering in the resources of chemistry means of securing the miner from the effects of the fire-damp.

"On considering the subject before I was acquainted with the nature of the gas, the simple method of burning a lamp in a pneumatic apparatus supplied with air through water occurred to me, as it probably has to every one versed in chemistry; but I found, on inquiry, that this idea had been long ago put into execution by Dr. Clanny, and published, whilst I was absent from England, in the Philosophical Transactions. Dr. Clanny showed me his lamp at Bishop Wearmouth, after I had made some inquiries as to the state of the mines. It appeared to me ingeniously executed; but when I proposed the trial of it to some enlightened and liberal inspectors of mines, they stated that its size, weight, and the manual labour or machinery required to work it, rendered it inapplicable for the common uses of the collieries.

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* A very interesting account of this event has been published by the Rev. John Hodgson. I have named this gentleman amongst many others who obligingly gave me assistance in my inquiries, and I cannot mention him again without again making my acknowledgements for the variety of information he afforded me during the visits that we made together to the collieries, and for the general interest he has taken in my experiments on the subject.

"I had a very portable lamp made on the principle of entire insulation from the atmosphere, into which the air was thrown, not through water, but by a small piston secured by valves; and in this I found I could detonate any gaseous mixtures made explosive by atmospherical air, without communication of the explosion to the external atmosphere: but finding during an investigation of the chemical properties of the fire-damp new and unexpected principles of security, I gave up all experiments upon the piston-lamp, as well as upon another contrivance of an entirely different kind described in the Appendix.

"The principles of security furnished to me by a philosophical inquiry into the properties of the gas are all derived from the general fact, that the fire-damp requires a much higher temperature for its combustion than other inflammable gases. Hence small additions of azote or carbonic acid destroy the explosive powers of mixtures of fire-damp and air; hence explosions of mixtures will not take place when their quantities are small compared to the cooling surfaces to which they are exposed; and hence one part of an explosive atmosphere of fire-damp may be burnt in free communication with another by certain cooling apertures or surfaces without any danger of explosion.

"As soon as I had fully developed the principles of security, which was towards the end of October, I had a lamp constructed, which I found was uniformly extinguished when introduced into explosive mixtures of the fire-damp, and I communicated my views on the subject in two papers read before the Royal Society, November 9 and November 16.

"The President of the Royal Society, with his accustomed zeal for the promotion of every useful public object, suggested to the Council of that Body, the propriety of giving speedy circulation to these inquiries, and the Council liberally sanctioned their immediate publication.

"I consequently condensed the two papers into one; added some improvements in the construction of the lamps, and gave to the investigations the form in which they appear in the following pages.

"Since the paper and the Appendix have been printed, the consideration of the principle has led me to a discovery which appears the most important in the whole progress of these researches.

"When I found that explosive mixtures admitted through narrow metallic canals brought in contact with flame, burnt only at the surface where they issued, I had hopes of keeping up a constant flame from explosive mixtures issuing from tubes or canals; but on trying this, even in atmospherical air, it failed.

"Conceiving that the failure was owing to the great cooling powers of the metallic sides of the canal, it occurred to me to try the metallic *wire-flame sieves*, and with these I had perfect success.

"I inclosed a very small lamp in a cylinder made of wire-gauze having 6400 apertures in the square inch. I closed all apertures except those of the gauze, and introduced the lamp burning brightly within the cylinder into a large jar containing several quarts of the most explosive mixture of gas from the distillation of coal and air; the flame of the wick immediately disappeared, or rather was lost, for the whole of the interior of the cylinder became filled with a feeble but steady flame, of a green colour, which burnt for some minutes, till it had entirely destroyed the explosive power of the atmosphere.

"This result, so satisfactory, immediately led to a number of experiments which gave results, if possible, still more satisfactory.

"I introduced the lamp inclosed in the cylinder, at different times, into large quantities of various explosive mixtures, some containing only one volume of coal-gas to four of air, and others containing one of coal gas to 13 of air. In all cases the flame was confined to the cylinder, and in all of them it continued till the mixture ceased to be explosive.

"In mixtures of 13, 12, and 11 parts of air to one of coal-gas, the flame of the taper did not disappear; it became paler, however, and blended with the flame of the explosive mixture filling the cylinder. As the quantity of the inflammable air diminished, the flame became limited to the wick, and was gradually extinguished. When there was as much as one of coal-gas to 7 or 8 of air, the flame of the taper was lost at first in the flame of the explosive mixture, which was very bright; but it appeared as the mixture became less explosive.

"When the coal-gas was 1 to 4 or 5, the flame of the wick never appeared in any part of the experiment; and the light of the flame from the mixture was weaker than in the other experiments.

"In taking the wire-cylinder and its lamp out of the explosive mixture, the flame of the lamp continued to burn in the atmosphere.

"In all the experiments the flame of the explosive mixture in the cylinder had more or less of a greenish cast, which is probably to be attributed to the effect of the brass wire.

"In one instance, in which a very large wick was burnt in a very small cylinder, the wire-gauze became red hot opposite to the wick at the first moment of the introduction of the cylinder

into

into an atmosphere consisting of about 1 of coal-gas and 11 of air; but it soon lost this temperature: and in other experiments made with smaller wicks in a dark room, I have seldom seen the wire dull red: but as no explosion ever took place in an atmosphere made explosive by coal-gas, the circumstance, for reasons that will be fully stated in the communication made to the Royal Society*, will hold good with much more certainty of the fire-damp.

"I own I expected an explosion in the instance when I saw the wire-gauze red hot; but the mass of heated matter was probably too small to heat considerably the portion of gas in contact with it, and the cool air must have entered principally at this part of the wire-gauze cylinder, and must speedily have reduced the temperature; and I have since found that a red-hot wire of less than the $\frac{1}{30}$th of an inch does not explode mixtures of coal-gas and air.

"When I took the wire-gauze lamp out of the most explosive mixture before it had consumed much of the inflammable gas, the mixture usually was explosive by the flame of the taper; but the explosive power of a mixture of 12 or 13 parts of air to one of gas was very soon destroyed by the combustion in the cylinder lamp; and even when it was withdrawn almost immediately, the taper burnt in the mixture merely with an enlarged flame.

"It is needless to dwell upon the practical applications of these facts; many of them will be immediately perceived. Wire-gauze may be substituted for horn or glass in the safe-lanterns or safe-lamps to be used in the collieries, and no air feeders below the flame will be necessary. The wire-gauze admits a free circulation of air, and it emits considerably more light than common horn. I have had small cylindrical caps of wire-gauze made to fit small lamps by a screw, which are almost as portable as a common candle without a candlestick; and which are trimmed and supplied with oil through safe apertures without the necessity of taking off the cap. A similar cap may be used with the common candles of the colliers introduced by an aperture made tight with moist pipe-clay.

"Brass-wire gauze of the proper degree of fineness is manufactured for the use of mills and for sieves. I found gauze which contained 3600 apertures in a square inch sufficiently fine to prevent explosion used as a cylinder; but it did not bear the proof of a concentred explosion from a close glass vessel. Gauze of 5000 apertures to the square inch stood, however, this severe test. I have generally used gauze of 6400 apertures; and I have seen plated wire-gauze, which, I am told, is sold at

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[*] See Phil. Mag. vol. xlvi. p. 444—58.

Edinburgh, so fine that the square inch contains 13,200 apertures.

"I have tried cylinders of the size necessary for the colliers, in explosive atmospheres contained in large glass vessels; the light given by a cylinder of seven inches high and two inches in diameter in the fire-damp mixed with 12 of air, is nearly as bright as that of the lamp in common air, and even the least brilliant flame would enable the miner to find his way, and, I think, would be sufficient for him to work by when he was very near it. The light is much increased by hanging within, from the top of the cylinder, a small cage of platina or iron wire; this becomes ignited by the flame, and gives a steady red light in the midst of it; and I have never had an explosion in employing it.

"The cheapness of the wire-gauze safe-lamps or guards for candles, will be an additional reason for bringing them into common use; for the dearest of them can hardly cost more than one shilling.

"They have the advantage of guarding the light from loose materials falling from the roof of the mine; there is no danger of their being broken; and to prevent them from being bent, they may be covered with a frame without, constructed of a few pieces of thick wire. The gauze should be made into the form of cylinders by double joinings sewn together through the double part by a wire of small diameter; any aperture larger than that of the gauze-wire should be most carefully guarded against, and the cylinders should be tried in a vessel filled with an explosive mixture before they are used in the mine. A coating of oxide is soon formed upon the brass; but in my limited experience this has appeared to defend the interior from the action of air, and to render the wire durable: if, however, it should be found that this does not hold good in the mines, thicker or plated wire may be adopted, or double cylinders, or, at all events, as the top of the cylinder is principally exposed to heat, this may be double.

"With the wire safe-lamp or guarded candle the miner may explore all parts of the mine where explosive mixtures exist, and the state of the flame will show him the degree of contamination of the air. As the fire-damp mixes with the air the flame will enlarge. When the fire-damp has reached its explosive point his cylinder will be filled with flame; but the flame of his wick will appear within the flame of the fire-damp. As the inflammable gas increases in quantity the flame of the lamp will disappear, and the flame in his cylinder will become paler; and this ought to be a signal to him to leave that part of the workings. For when the flame of the fire-damp is extinguished, though the air may be sufficiently respirable to enable him to make good his way, yet it cannot be breathed safely for any time.

"I found that sparks from steel and flint fell red, and without scintillation, in a mixture of coal gas in which a cylinder lamp had burnt out; and they appeared equally dull and red in a mixture of three parts of air and one part of coal gas; so that the light of a steel mill would not be sufficient to work by in an atmosphere in which the cylinder lamp was extinguished, and it could only be employed to guide the miner out of an atmosphere which it would be fatal to breathe for a continuance.

"To conclude: there appears every reason to expect that the safe light, in this state of improvement, with proper attention, will enable the miner to work with perfect security in parts of the mines most liable to fire-damp, and that it will not only preserve him from, but enable him to combat and subdue, his most dangerous enemy. Confined in the wire-gauze safe-lamp, the flame of the fire-damp will be divested of all its terrors, and made to expend energies formerly so destructive, in producing an useful light.

London, Dec. 31, 1815.

Note.

"A considerable degree of heat is *always* produced by the combustion of the explosive fire-damp in the wire-gauze cylinders; therefore a candle soon melts away in the lantern, when the fire-damp is burning in it; and if candles are used with the wire-gauze safe-guard, the flame of the fire-damp should be extinguished by putting a woollen or linen extinguisher over the cylinder, to prevent the candle from dropping out, or the candle should be secured in the bottom of the lantern, by a safety screw: where the fire-damp is known to exist, it will, however, always be better to work with small lamps, which may be fed with tallow; and where the object is to destroy the fire-damp speedily, a large cylinder lamp with double wire-gauze may be used.

"The joinings in the lamps should be made rather with hard than soft solder, and there should always be a handle at the bottom, or a ring at the top, to prevent the hand from being burnt.

"The flame of the fire-damp in the cylinders may be easily extinguished by a cover made of coarse paper, or by a woollen cap. If any part of the wire is found to become strongly red hot, water may be thrown upon it, or the communication may be interrupted by plates of metal.

"Many devices may be contrived for giving light by the fire-damp; lamps may be made partly of glass and partly of wire-gauze: and by making a chimney partly of metal, the fire-damp may be burnt only at certain surfaces.

"When the cylinder lamp is first introduced into an explosive atmosphere, a musical sound is produced, like that produced by hydrogen burning in narrow tubes.

"I have since found that the size of the apertures may be carried to 900 in a square inch, and that the wire may be 1-70th of an inch in thickness, and these probably are not the limits. The larger the apertures of the wire-gauze the greater the heat produced, and the more brilliant the flame, so that with very coarse wire-gauze it will be proper to interrupt too free a circulation of air by using a tin-plate cylinder which will act as an extinguisher, either for a part or the whole of the flame; and for security, it may be proper to adopt double wire cylinders; perhaps iron wire will be better than brass. It will be easy by various means to keep the tin-plate cylinder below the red heat, though probably the temperature will never approach this.

Elements of Electricity and Electro-Chemistry. *By* GEORGE JOHN SINGER. 1 vol. 8vo. with Plates by Lowry, 507 pp. Longman and Co. Price 16s.

The rapid progress of electrical discovery since the invention of the battery of Volta, and the present importance of electricity as an essential branch of chemical philosophy, are circumstances which render an elementary work on this subject adapted to the existing state of knowledge, an acquisition of real value: and as the brilliant series of discoveries which have so eminently distinguished the commencement of the 19th century have not before appeared in any regular treatise on electricity, we notice with pleasure the successful endeavours of Mr. Singer to lessen the difficulties and promote the cultivation of an interesting department of science by a clear and comprehensive statement of its most essential facts.

The author has paid very considerable attention to the arrangement of his subject, and by this means has effectually promoted his object of rendering it more familiar. We extract the following remark on this subject from the preface to the work. "By a proper attention to arrangement I have been enabled to communicate a more extensive collection of facts in a single volume, than is to be found in any existing treatise with which I am acquainted; and that attention may be expected to render even more concise statements amply intelligible; for materials thus disposed are like the combination of stones in an arch; they mutually support each other, and form a connected series in which every part is essential to the existence of the whole."

The details in this work are certainly more intelligible and less prolix than in the majority of those on the same subject which have come under our observation; and yet they involve a more extensive series of phænomena. This advantage is most probably derived partly from the extensive practice of the author as a lecturer, and partly from the assiduous attention which he has paid to the science from a very early age. The subject is introduced by a historical sketch of the principal epochs of discovery, which is calculated to prepare the reader for the scientific exposition of the science. The work itself is divided into four parts, which are again subdivided into chapters. The first part describes "electrical phænomena and the circumstances essential to their production." This part is divided into five chapters, arranged as follows:

Chap. I. Nature of electrical action and sources of electrical excitation; positive and negative electricity.

Chap. II. Of conductors and non-conductors of electricity, and of the electrical apparatus.

Chap. III. Experiments with the electrical machine.—Theory of its action.—Phænomena of attraction and recession.

Chap. IV. On the phænomena of electric light.

Chap. V. On the Leyden jar and the nature of electrical influence.

This part of the work is chiefly remarkable for the distinct arrangement which it presents, and for a careful selection of experiments which are explained very clearly by a judicious modification of Dr. Franklin's theory. The distinguishing characters of electrical action are described as consisting in the attraction of light substances and the emission of light, the repulsion or separation observed between electrified substances being ascribed by the author to the attraction of the surrounding air or other medium, and not to any repulsive power, the existence of which, he asserts, is purely hypothetical. In this opinion he is supported by the authority of Kinnersley, Morgan, Volta, and De Luc, and by the experiments of Lord Stanhope, and it certainly renders the explanation of many phænomena much more simple and satisfactory.

The chief sources of electrical phænomena are; 1st, Friction. 2d, Change of form. 3d, Change of temperature. 4th, Contact of dissimilar bodies. It is the last which our author considers as in all cases the primary cause of electrical excitation; for it occurs in all the others; and in the most simple case of electrical action, that of De Luc's electrical column, the contact of dissimilar bodies is the only known cause of the effects produced.

In the table of the effects of excitation, some corrections of the results given by Cavallo are made; but the author has not rendered it either so complete or perfect as we were led to expect from our knowledge of the extent of his experience.

A variety of experiments are adduced to prove that positive and negative electricity are always produced together, and the phænomena resulting from the action of a Nairn's machine are minutely described as affording the following indications:

1. That the cause of electricity is corporeal; for sensation is affected by it, and a mechanical impulse is experienced.

2. That there is a mutual action between the electricities excited in the opposite conductors, since their effects are more powerful when directed at the same time to our conducting body.

3. That the same relation which is observed between the opposite conductors exists also between either of them and the ground, but in a different degree. From these phænomena the following propositions are deduced:

1. The cause of electrical phænomena is material, and possesses the properties of an elastic fluid.

2. The electric fluid attracts and is attracted by all other matter, and in consequence of such attraction exists in all known substances.

3. The attraction of different bodies for the electric fluid is various, and so is that of the same body under different circumstances: consequently the quantity of electricity naturally existing in different substances may be unequal; and the same body may attract more or less than if alone when combined with other matter: but its original attraction will be restored by destroying the artificial combination.

4. From some peculiarity in the nature of the electric fluid, its attraction by and for common matter is more influenced by figure than by mass; and is consequently stronger in extensive than in limited surfaces.

5. From the same peculiarity, the electric fluid moves with great facility over the surface or through the substance of some bodies, and is arrested in its progress by others.

6. When the attraction of any substance for electricity is equal to the electric fluid it contains, that substance will evince no electrical signs; but these are immediately produced when there is either more or less electric fluid than is adequate to the saturation of the existing attraction: if there be more, the electrical signs will be positive; if less, they will be negative.

This leads to the following theory of excitation. "The bodies employed have each a certain quantity of the electric fluid proportioned to their natural attraction for it. This they retain, and appear unelectrified so long as they remain in their natural state. Now if two such bodies are brought into contact, their natural attractions are altered, one of them attracts more than in its separate state, and the other less; the electric fluid diffuses itself amongst them in quantities proportioned to their relative attractions, and they consequently appear unelectrified. But if they are suddenly separated, the new distribution of the electric fluid remains, whilst the original attractions are restored; and as these are not equal to each other, the bodies will appear electrical; that whose natural attraction was increased by contact, having received an addition to its quantity of electric fluid, will be positively electrified; and that whose attraction was lessened, having lost a portion, will be negative."

The action of the electrical machine is explained very satisfactorily by this theory. Its effect is to bring successively in contact with the rubber different parts of the surface of the glass, which are as suddenly separated from that contact, and carry with them the electricity they have acquired by the change of capacity it produces.

The same theory is applied very naturally to all the usual effects of electric motion and the action of different conductors in transmitting electricity: the explanation of points appears to us novel and appropriate. It is stated to consist in promoting the recession of the particles of electrified air, by protruding a part of the electrical atmosphere of the conductor into a situation more exposed to the action of the ambient unelectrified medium, and thereby producing a current of air from the electrified point to the nearest uninsulated body.

The phænomena of electric light are treated at some length, and explained in a very simple manner; the Leyden jar, the condenser, and the electrophorus, are referred to the action of one principle, that of electrical influence, which has been so well illustrated by Volta in his excellent paper in the lxxiid volume of the Philosophical Transactions. The explanations differ essentially from those of most preceding writers. The author uniformly avoids the use of repulsion as a principle of electrical action, but he does not adopt to any extent the opinions of those who have preceded him in this idea.

The second part of the work treats of the mechanical and chemical agencies of electricity, and contains three chapters.

Chap. 1. describes the instruments required for the application of the electric power to the purpose of experiment.

Chap. 2. the mechanical agencies of electricity.

Chap. 3. chemical effects of electricity.

The description of instruments is concise; it includes only those which are really necessary; it contains much useful information, and concludes with the following very just remark: "Tubes of glass, wires of different metals, corks, and a few other materials, are adequate to the construction of an endless variety of electrical machinery, and the proper direction of such resources is constantly followed by useful discovery. Mechanical dexterity is therefore essential to the character of an electrician, since his progress will be in proportion to the facility with which he can adapt the objects around him to new inquiries. He cannot deviate from the beaten track of his predecessors without the aid of new combinations; and when the supply of these is derived from his own industry and ingenuity, the ardour of his pursuit will be unimpeded by the delays or mistakes of others; and the projection of any required improvement may consequently be followed by its immediate consummation."

In describing the mechanical effects of electricity, Mr. Singer has particularly enlarged on the expansive effects it invariably produces when passing from one conductor to another, and has very successfully applied the observation of this effect to explain the results obtained by Mr. Symmer, when a charge is passed through several sheets of tin-foil placed between the leaves of a paper book, which were considered as an indication of the course of the electric fluid. As Mr. Singer has published a separate paper on this subject in The Philosophical Magazine, it is unnecessary for us to enter more fully on the subject. The following experiments will probably be new to many of our readers. "Colour both sides of a card with vermilion, and place it upon the table of the universal discharger; one of the wires should be beneath the card, and the other in contact with its upper side; the distance of the points of the wires being one inch. If a charge be now passed through the wires, the fluid will pass from the positive wire across the surface of the card to the part over the negative wire, and it will perforate the card in its passage to the negative wire. The course of the fluid is permanently indicated by a neat black line on the card, reaching from the point of the positive wire to the hole; and by a diffused black mark on the opposite side of the card around the perforation, and next the negative wire. These effects are very constant, the black line always appearing on the side of the card which is in contact with the positive wire, and the perforation being near the negative wire."

Mr. Singer has also found that a light float-wheel, made of card paper, and placed between two oppositely electrified points, will move from the positive to the negative; and that a piece of card paper supported vertically by a narrow cork base in a similar situation, is thrown down also from the positive towards the negative.

The experiments appear to us amongst the most satisfactory that have been offered in evidence of a current of electricity from the positive to the negative surface, but we do not regard them as absolutely decisive.

The chemical agencies of electricity include only the effects of common electricity, the action of the Voltaic apparatus being reserved for the concluding part of the work. The action of the electric charge on metals is treated at some length, and the description involves considerable practical information on the means of conducting such experiments. The following is a simple means of reviving metals from their oxides:

"Introduce some oxide of tin into a glass tube, so that when the tube is laid horizontally the oxide may cover about half an inch of its lower internal surface. Place the tube on the table of the universal discharger, and introduce the pointed wires into its opposite ends, that the portion of oxide may lie between them. Pass several strong charges in succession through the tube, replacing the oxide in its situation, should it be dispersed. If the charges are sufficiently powerful, a part of the tube will soon be stained with metallic tin, which has been revived by the action of the transmitted electricity.

The decomposition of water and the action of electricity on different gases are also fully described, and are accompanied by a table of the results of electrifying mixed gases; and another on the action of electricity on compound gases. On the causes of these phænomena, which the author does not attempt to explain, he has the following observations:

"These various effects produced by the same agency do not appear susceptible of any other explanation than that which assumes the action of electricity to be mechanical; and even on this assumption they are not strictly intelligible. The momentary agitation into which the various mediums are thrown by the action of the spark, might be considered as likely to promote a new arrangement of parts; but, admitting this, why is the change instantaneous in some instances, and gradual in others? And by what inversion of principle is the same impulse that unites the particles of bodies, enabled subsequently to separate them? These are questions it would be interesting to resolve; but there appears no clue by which such intricate processes can be at present analysed. The chemist must therefore be content to avail himself of the practical advantages they afford to his art, and await the progress of discovery for the development of their theoretical relations."

An enumeration of the bodies which are rendered phosphorescent by electricity, with remarks on that singular phænomenon, and a sketch of the magnetic effects of electricity, close this part of the work, which the chemical student will find a very interesting and instructive exercise.

[To be continued.]

Mr. William Phillips has in the press, An Elementary Introduction to the Knowledge of Mineralogy and of Minerals; including some account of the places at which, and of the circumstances under which, minerals are found; and explanations of the terms commonly used in mineralogical description.* It is designed for the use of the student, and will be comprised in a small volume in dudecimo, which it is expected will be published in the course of the month.

Dr. Granville has in the press, and nearly ready for publication, A Translation of that Part of Orfila's general Toxicology, which more particularly relates to poisons from the vegetable and animal kingdoms. The subject having formed a very immediate branch of Doctor Granville's scientific pursuits, he has been enabled to accompany his translation with copious notes and additions.

The original has only been before the public a few days, and is not yet in general circulation.

Origin of Nations.—In February 1816 will be published, A Map of Scriptural and Classical Geography; accompanied by an Historical and Descriptive Volume, in demy octavo; wherein the Origin of Nations is particularly examined and discussed; with reference to the numerous Authorities: amongst which Herodotus, Hesiod, Strabo, Pliny, Diodorus Siculus, Cluverius, Ptolomy, Mela, Bochart, &c. have been expressly consulted. The whole is intended to facilitate a knowledge of the progressive Colonization of the Earth; and to establish, more clearly, the Foundation of Universal and Chorographical History: and also to combine a requisite appendage to every volume of the Ancient Classics, with an indispensable Auxiliary to the Sacred Memoirs of the Holy Scriptures. By T. HEMING, of Magdalene Hall, Oxford.

The size of the Map will be three feet two inches, by two feet one inch; including, in longitude, from the Meridian of London to the Eastern Boundary of Persia; and in latitude, from the Northern Coast of Africa to the Southern Shores of the Baltic and Middle Regions of Russia, which contains all that is essentially necessary to illustrate a Course of History from the first colonial migrations of mankind, to the overthrow of the Roman Empire, on a scale of three quarters of an inch to a degree.

The projection of the Map is the *globular,* which best preserves the proportion of the different countries; and it was carefully divided into half degrees throughout, in order to lay down the coasts, rivers, mountains, &c. the more accurately, and to have the basis of the work correspond to the best corrected topography of the present time.

The Map will be published by W. Phillips, George-Yard, Lombard-Street.—Price, including the Historical and Descriptive Volume, One Guinea, to be paid on delivery.

Mr. C. Taylor has commenced the publication of "EGYPT," consisting of "a series of 100 engravings exhibiting the antiquities, architecture, inhabitants, costume, hyeroglyphics, animals, scenery, &c. of that country, with accompanying descriptions and explanations in French and English. Selected from the celebrated work detailing the expedition of the French, by Baron Vivant Denon."

The engravings are executed by Middiman, Cardon, Cooke, Roffe, &c. The work is to be comprised in twenty numbers, large folio, at five shillings each. Three numbers of this splendid and highly curious work have already appeared, and their contents and execution cannot fail to obtain public approbation. To astronomers the curious planisphere in the Temple of Tentyris must be peculiarly interesting, as it proves "that the Egyptians had a planetary system, and that their knowledge of the heavens was reduced to principles." Egyptian locks, which fastened the gate of a town, the door of a house, or the smallest box or drawer, furnish specimens of these ancient people's skill in mechanics; while the portraits of the different distinguished characters which Denon had so good an opportunity of taking in Egypt, will amuse all persons of taste and inquiry respecting human character and physiognomy.

A Chemical Chart or Table, exhibiting an elementary View of Chemistry, intended for the Use of Students and young Practitioners in Physic; also to revive the Memory of more experienced Persons; adapted for hanging up in public and private Libraries. Dedicated, by Permission, to George Pearson, Esq. M.D. F.R.S. &c. &c. By Robert Crowe, Surgeon R.N. Price 5*s*. 6*d*. Highley and Son. 1816.

Several attempts have been made to reduce a knowledge of the science of chemistry into a tabular form, but many discoveries have been made, and the whole system considerably modified, since their publication. Mr. Crowe, therefore, in availing himself of preceding works, arranging the subjects somewhat more simply, and including all the recent discoveries, has contributed to facilitate the acquisition and recollection of this most interesting science. He divides his Chart into three sections: the first describes the earths and metals; the second treats of simple or undecompounded substances; and the third, of compound combustibles. In a distinct compartment the author has arranged a very distinct view of the minerals, vegetable and animal acids, which will be found very convenient for common reference.

A Prospectus has just appeared, announcing for publication upon the 31st of March the first number of a Quarterly Journal of Science and the Arts, and that it will be regularly continued upon the last days of March, June, September, and December. This Journal will contain a series of original communications upon subjects connected with science and the arts, and with philosophical literature in general. Notices of scientific discoveries and inventions, and of experiments and researches carried on in the Institution. Reviews and notices of scientific works Abstracts from the Transactions of learned societies, and from domestic and foreign publications; and accounts of the proceedings of the members of the Royal Institution, and of the public and other courses of lectures.

The name of Mr. Brande, of the Royal Institution, to whom communications are requested to be addressed, is a pledge that this work will be conducted on liberal and enlightened principles; and we hope it will meet with due encouragement.

Dr. Henning, of the Hot-Wells, Bristol, author of an Inquiry into the Pathology of Scrofula, is preparing for the press a work on Pulmonary Consumption, which will be ready for publication early in the spring.

Extract of a French Work entitled "L'Optique des Couleurs," (the Optics of Colours). By Father Castel, *a Jesuit.*

I know a painter whose taste and talent in portrait-painting I esteem very much, and who, in showing me his painting-room, which was very poor in colours, made me expressly observe, that there was neither carmine, nor lake, nor vermilion for the reds, nor any lively yellow; but simply Prussian blue for the blues and greens, a brown red for all sorts of reds and violets, and a very indifferent yellow, the name of which I have forgotten.

His portraits were very fine. His flesh colours were more especially very natural, very lively, and very bright even, when it was required.

I reasoned with him, and objected to him that other celebrated painters did not fail to employ the most lively reds and the brightest yellows. He agreed with me that paintings thus coloured, more particularly with red, were much in fashion. He brought me back, however, to truth and to immortality. Those colours, he observed to me, are false; nature is lively by contrast alone, and by the judgement she displays. All her colours are but indifferent in the detail of each trait; but it is the opposition which brings out her productions, and gives them life, fire, and the greatest *éclat*. He added that lake, carmine, vermilion, and other *sharp* colours, had not any body, and did not hold for a long time, observing that those who employed them did not work for immortality.

These lively colours are in reality too pure: to give them body and consistence, and at the same time to imitate nature—beautiful nature—they should be soiled.

From all these observations I am of opinion, that to paint and dye with effect, or, in a word, to colour scientifically and on regular principles, such as I think are here developed or sketched by me, all the reds, such as lake, carmine, and vermilion, the red brown, minium even, should be taken in the mass, and by their admixture an universal red produced.

The just proportions of this mixture would be found by degrees, and indeed very soon. It would certainly form a very fine red—an intermediate red, tempered, primitive, full, embodied, harmonious, certain, and durable. I am led to think that the brown red should predominate, and constitute its body and principal basis.

The same may be tried on the blues and yellows: there would thus be three good colours, from which all descriptions of pure and dingy tints of colouring would be produced, by blending them two by two, or all the three.

A dark colour bordering on black might even be procured by this mixture; and who knows but that a white, or whitish gray, may not be formed by blending certain colours naturally clear, such as are to be found among the yellows, and even in a certain degree among the reds?

From art let us proceed to nature. I have observed that the greater part of the colours employed by her wisely sparing hand are also dingy colours, into the composition of which many kinds of colours enter. It is very rare to find in flowers, in shells, and more particularly in animals, true simple colours, or which do not result from the mixture of more than two simple colours.

This fact being once ascertained, it was not difficult for me to enter into the reasons of nature. In the first place, the physical and practical reason is the same as I have before assigned, that every thing on the earth, and within the earth, is much blended; and that all bodies, plants, minerals, and animals, are in reality *mixed.*

But the causes of design, the final causes of nature, or of her all-wise Author, merit at least as much or still greater attention. The pure colours are very limited in their number, the dingy colours are much more extensive, and form a much greater variety—the only source of our delight.

It is in this way that the verdure of the plains, which may be thought so monotonous and uniform, is not at all so. If it were throughout a fine green, a true green, it would in reality be a very insipid monotony. Nothing, however, is more diversified, by the different touches of red, violet, and orange, which are manifestly blended with this green, as if to soil it, and which render it rich, and infinitely charming,—on this account alone, that it is diversified.

One of the reasons why nature is lavish of her dingy colours, is to give a brilliancy to the true colours, either by their rarity, or by the contrast. If we examine closely, we shall seldom see a fine colour which is not contrasted, in natural objects, by obscure colours: these become, however, precious, by the contrast even which they oppose to the colours that are precious in themselves, and with which they are assorted.

The green of the leaves of orange, lemon, and pomegranate trees is considered as a fine green, and is every thing but that if we view it attentively. The fact is, that we constantly represent it to ourselves in the point of view of these golden fruits, or of these pomegranate flowers, which, while it is relieved by them, gives a fresh *éclat* to their beauty.

I have known a person who, in a picture of contrast, boasted of the shades, as if they had been the finest colours in the painting, saying expressly that he had never seen such fine colours as these shades. He confounded the brightness which they cast on the coloured parts, with the shades themselves. Whatever contributes to the beauty of any thing, partakes in a certain degree of this beauty. This is because every thing is relative, and because a fine conformity renders the beauty reciprocal to each of the terms of the comparison, although one of them may be a negative beauty, oftentimes founded on a positive uncomeliness.

XV. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

Jan. 11. Sir H. Davy communicated a short paper detailing some further Experiments on Fire-damp. Sir H. has advanced from discovery to discovery in the most effectual mode of preserving the lives of miners at the least possible expense. It appears from these experiments, that no new lamp or other apparatus is necessary to prevent explosions; that the lamps now in use, when covered with a wire-gauze screen, are not only perfectly sufficient to preserve the miners from all danger, but even may be used to consume the fire-damp by burning it to show them light. By surrounding the lamp with a fine wire-gauze screen, saturating the screen with fire-damp and inflaming the whole; the wire if fine, and the apertures not exceeding $\frac{1}{30}$th of an inch, may be made red hot without exploding the circumambient fire-damp. With a small portion of fire-damp in the screen the flame of the lamp is visible; but when a considerable portion is thrown into it, the whole becomes one entire flame. In this manner the carburetted hydrogen gas may be burned under the screen without the least danger of exploding the gas around it.

The conclusion of Dr. Brewster's curious paper On the Radiation of Heat in Glass Plates was read; but from the multiplicity of the experiments, and the necessity of diagrams to illustrate them, it is impossible to convey any adequate and intelligible idea of the new and important results. It appears that this ingenious and indefatigable philosopher has succeeded in contriving and arranging glass plates so as to form a new kind of thermometer, by making a series of plates (amounting to 20) of dimensions varying in a regular gradation. The apparatus is simple, and may be adapted to the performance of many pleasing experiments, as well as developing some singular phænomena in optics.

Jan. 25. Another paper by Sir H. Davy was read, containing an account of some more successful experiments in the cause of humanity and the interests of Coal Miners. It appears that Sir H's invention has already been adopted in two coal-mines with the most complete success; thus at once falsifying all the ignorant and invidious predictions of inhuman speculators about its inutility, and demonstrating, what every reasoning man of science before believed, that the invention would be equally effectual and practicable. There is now every reason to believe that Sir H's plan will be universally adopted without the least delay. No man or society of men, indeed, would now risk the responsibility consequent on an explosion, when the means of avoiding it are both known and so easily adopted. The present paper contains a variety of experiments to ascertain the smallest number of apertures in a square inch which can be used without danger of exploding. Wire-gauze having apertures $\frac{1}{10}$th of an inch, when the wire became red hot, exploded; but gauze with apertures only of $\frac{1}{30}$th were perfectly secure even with the greatest heat. In some of his experiments, Sir Humphry used gauze having 6000 apertures in a square inch, which was found as perfectly secure as a brick wall could have been against explosion.—*See pages* 54 & 56.

Part of a long paper, by Dr. Wilson Phillip, was read; detailing the result of numerous experiments on the Nerves of

Rabbits exposed to the Action of the Galvanic Fluid. In some experiments the rabbits were first pithed, and then galvanism applied to ascertain how long and in what degree the vital functions could be revived. It was, however, candidly admitted that no legitimate conclusions could be drawn from experiments made on animals under the circumstances of pain, disease and derangement occasioned by the previous wounds.

ROYAL INSTITUTION.

The following is the Plan of an extended and practical Course of Lectures and Demonstrations on Chemistry, to be delivered in the Laboratory of the Royal Institution by William Thomas Brande, esq.

These Lectures will commence on Tuesday the 6th of February at Nine in the morning, and will be continued every Tuesday, Thursday, and Saturday.

The subjects comprehended in the Course are treated of in the following order:

Division I. *Of the Powers and Properties of Matter, and the general Laws of Chemical Changes.*

§ 1. Attraction—Crystallization—Chemical Affinity—Laws of Combination and Decomposition.—§ 2. Light and Heat—Their Influence as Chemical Agents in Art and Nature.—§ 3. Electricity—Its Laws and Connexion with Chemical Phænomena.

Division II. *Of undecompounded Substances and their mutual Combinations.*

§ 1. Substances that support Combustion, Oxygen, Chlorine, Iodine. — § 2. Inflammable and acidifiable Substances—Hydrogen—Nitrogen—Sulphur—Phosphorus—Carbon—Boron.—§ 3. Metals—and their Combinations with the various Substances described in the earlier part of the Course.

Division III. *Vegetable Chemistry.*

§ 1. Chemical Physiology of Vegetables.—§ 2. Modes of Analysis—Ultimate and proximate Elements.—§ 3. Processes of Fermentation, and their Products.

Division IV. *Chemistry of the Animal Kingdom.*

§ 1. General views connected with this department of the Science.—§ 2. Composition and Properties of the Solids and Fluids of Animals—Products of Disease.—§ 3. Animal Functions.

Division V. *Geology.*

§ 1. Primitive and secondary Rocks—Structure and situation of Veins.—§ 2. Decay of Rocks—Production of Soils—Their analysis and principles of Agricultural improvement.—§ 3. Mineral Waters—Methods of ascertaining their contents by Tests and by Analysis.—§ 4. Volcanic Rocks—Phænomena and Products of Volcanic Eruptions.

In the first divison of each course, the principles and objects of chemical science, and the general laws of chemical changes are explained, and the phænomena of attraction, and of light, heat, and electricity developed, and illustrated by numerous experiments.

In the second division, the undecompounded bodies are examined, and the modes of procuring them in a pure form, and of ascertaining their chemical characters, exhibited upon an extended scale.—The lectures on the metals include a succinct account of mineralogy, and of the methods of analysing and assaying ores.

This part of the courses will also contain a full examination of pharmaceutical chemistry; the chemical processes of the Pharmacopœiæ will be particularly described, and compared with those adopted by the manufacturer.

The third and fourth divisions relate to organic substances.—The chemical changes induced by vegetation are here inquired into; the principles of vegetables, the theory of fermentation, and the characters of its products are then examined.

The chemical history of animals is the next object of inquiry—it is illustrated by an examination of their component parts, in health and in disease; by an inquiry into the chemistry of the animal functions, and into the application of chemical principles to the treatment of diseases.

The courses conclude with an account of the structure of the earth, of the changes which it is undergoing, of the objects and uses of geology, and of the principles of agricultural chemistry.

The applications of chemistry to the arts and manufactures, and to œconomical purposes, are discussed at some length in various parts of the courses; and the most important of them are experimentally exhibited.

The Admission Fee to each Course is Four Guineas; or by paying Eight Guineas, gentlemen are entitled to attend for an unlimited time.

Further particulars may be obtained by applying to Mr. Brande or to Mr. Fincher at the Royal Institution, 21, Albemarle-Street.

RUSSELL INSTITUTION.

A Course of Lectures on Electrical Philosophy, with its application to the improvement of Chemical Science, and the explanation of Natural Phænomena, will be commenced at this Institution by Mr. Singer, on Monday the 5th of February, punctually at Eight o'clock in the Evening.

These Lectures will be continued on the succeeding Mondays at the same hour:—they will embrace the most important features of this interesting branch of Natural Philosophy, with occasional observations on the Sciences with which it is most immediately connected.

Tickets and a Prospectus may be had of the Secretary at the Institution, Great Coram Street, Russell Square.

XVI. *Intelligence and Miscellaneous Articles.*

The Natural History Society of Geneva has invited the naturalists of the whole of Switzerland to attend a public meeting at Geneva, in order to lay the foundation of a general society under the name of "The Helvetic Society for the Natural Sciences."

M. Badeigts Laborde, a seafaring gentleman of the department of the Landes in France, has ascertained by repeated experiments, that the resinous trees of France are capable of yielding rosin and tar not inferior in quality to those which are brought from the north of Europe at a great expense. M. Laborde has also ascertained that the French products contain the same constituent parts with those of Sweden; and that their inferiority is entirely owing to the imperfection of the furnaces in which they are prepared, their defective preparation, and their mixture with a certain quantity of water and heterogeneous matters, and particularly from the want of essential oil, which is burnt in the French operation for extracting rosin and tar. Finally, it is easy to purify the French articles so as to render them equally perfect, and at the same time far cheaper than those of Sweden.

The public will be gratified to learn, that the British Ministers are prepared to avail themselves of the opportunity afforded by the restoration of peace, to attend to the interest of science, and add to the stock of general knowledge. A small expedition is now about to proceed to explore the course of the Congo, in the south of Africa. A transport, accompanied by a steam-boat, will proceed to the mouth of the river, where it will remain while the last-mentioned vessel is dispatched to follow the course of the Congo, to ascertain how far it is navigable, and the character of the inhabitants of its shores; as also that of the animals and the various articles of commerce which that part of the world may produce. Every precaution has been taken to guard against the object of the expedition being disappointed. "The Congo" (the vessel which is to proceed up the river is so named) does not draw more than four feet of water. When it shall be found impracticable to proceed further in her, the undertaking will be confided to two small cutters which are joined together, the masts and sails being stacked between them so as to leave the navigators the full range of each, and these will not draw more than eighteen inches of water. Such arrangements give fair promises of ultimate success; but that which gives us most hope is the care taken to man this little expedition in the best possible manner. The hands to be employed, in number about 50, are all volunteers. None but those who are proved most efficient are accepted. Their exertions are stimulated by double pay. The officers are selected for their merit alone, and promotion is promised to those who may return. A trial of the vessels above mentioned, which are intended to be employed for the purposes above described, will shortly take place on the Thames. They will leave England early in the present month under the command of Captain Tuckey. It is expected it will take from eighteen months to two years to accomplish the objects of the voyage. Some natives of that part of Africa have volunteered their services, and, it is highly probable, will prove of great advantage. One represents himself as born more than 300 miles up the Congo, at a village on the banks of that river; and another as still further up. They speak the native language with fluency, as also English, and, it is said, have given some valuable information of the several kingdoms through which they must pass.

STEAM-ENGINES IN CORNWALL.

According to Messrs. Leans' Report for December, the average work of thirty-three engines was 19,335,126 pounds of water lifted one foot high with each bushel of coals consumed. During the same month the work done by Woolf's engine at Wheal Vor was 46,907,795, and that at Wheal Abraham, 47,622,040 pounds of water lifted to the same height with each bushel of coals.

REUNION OF SEPARATED PARTS.

Our readers, particularly those of the medical profession, will probably recollect that Dr. Balfour of Edinburgh published in 1814, in the Edinburgh Medical and Surgical Journal, two remarkable cases of adhesion of amputated fingers. A similar successful operation was performed lately in Scotland, as appears by the following interesting communication to Dr. Balfour by Thomas Hunter, Esq. Surgeon, Port Glasgow.

"January 6, 1815.—John Galbreath, aged 45, by trade a house-carpenter, in the act of hewing wood with an axe (which he held in his left hand) struck off his thumb close to the articulation of the first phalanx with the metacarpal bone. Being confused by the accident, he covered the stump with his other hand, and, accompanied by the foreman of the work, arrived about eight minutes after at my house. Upon examining the parts, I found the portion of thumb he had supposed cut off, lying in the hollow of his hand, buried in coagulated blood, and still attached by a portion of skin not exceeding one-eighth of an inch in breadth.

"Had I not been acquainted, through the medium of the Edinburgh Medical Journal, of your success in re-uniting separated parts, I certainly without the least hesitation would have cast this part from me, and proceeded to dress the stump; but I resolved on saving it if possible.

"Having carefully cleaned the parts, and removed a small splinter of bone, I replaced them, securing them with three stitches. After covering the thumb with dressing-lint, I placed a splint of wood above all, extending from the wrist to a little beyond the point of the thumb, and secured the whole, as neatly as possible, with a narrow ribbon. Finally, wetting the whole with tinctur. benzoin. comp. I ordered him to call frequently, that I might have the opportunity of ascertaining whether adhesion would take place.

"I examined the parts daily. He felt no pain—no discharge took place; no smell was perceivable. In this way he passed the time without ever having the first dressing touched, till the twentieth day from the accident, when I became so anxious to see the parts, that I undressed the hand, and, to my great satisfaction, found all skinned, one place, where there was a stitch, excepted. This was dressed daily, and soon healed. The splint of wood was continued for some time after, to give support to the bone. The man has been working for some months past, enjoying all the advantages of his thumb, only the motion of the joint is impaired.

"The above is a just statement of the case, and the patient signs it with the hand that suffered the injury.

(Signed) "John Galbreath, Patient.
"Wm. Cubrie, Foreman to the Work.
"Thomas Hunter, Surgeon."

THE BAZAAR.

The origin and commencement of such an establishment in London deserves to be recorded. The greater part of our readers must have seen some account of it in the daily newspapers: our notice shall therefore be brief at present. When more completely matured, and the whole put in motion, we may again return to it.

The British Bazaar owes its origin exclusively to the active benevolence and philanthropy of John Trotter, Esq. of Soho-Square, a gentleman well known in the higher circles, and to whom the nation owes many arrangements which have so much improved the circumstances of the British army when on active service in the field. Mr. Trotter has not merely the merit of contriving and arranging the establishment—but of providing the entire means for carrying it into full operation. The name adopted is, as our readers know, applied in the East to large markets, many of them covered, in which the merchants expose their goods to sale*; different portions being occupied by different traders. Mr. Trotter has devoted his extensive premises in Soho-Square to the accommodation of an extensive, industrious, but distressed class of the community, whose narrow circumstances keep them in obscurity, and preclude the possibility of their exhibiting for sale, in shops of their own, the various products of their industry. He has fitted them up in the most commodious and elegant manner, with counters, drawers, &c. With the lighting, warming, ventilating, and watching the premises the temporary occupiers will have no concern; but are to pay by the length of counter they may respectively occupy, at the low rate of one-fourth part of a shilling per foot, for each day they may require the accommodation. When their little stock is sold off their expenses terminate—the family prepare a new supply—they know where they may be again accommodated, and where no recommendation is wanted but that of an irreproachable character.

So far as regards general accommodation, collecting the various productions of art and ingenuity into one focus, and the civility and beneficial rivalry excited by such an assemblage, the new establishment will resemble those of the East; but here they cease to have any thing in common. In every other point of view the British Bazaar, like the British System of Education, the British and Foreign Bible Society, and the British Saving-Banks, will be found adapted to lay the foundation of a new system, which, we doubt not, will in a few years extend itself not only to every large town in the British dominions, but throughout Europe.

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* The Bazaar of Tauris is of such an extent that it has more than once afforded cover for 30,000 men ranged in order of battle! It is very probable that some of those extensive ruins still left at Palmyra, and other ancient cities now desolate, which have been hitherto considered as temples, or the palaces of royalty, are the remains of covered Bazaars. The term Bazaar is neither more nor less than the Hebrew word בזר, which means to *scatter*, to *disperse*—applied in Arabic to the merchant, *the disperser*, and hence to the place for *the dispersion* of his commodities.

ANTIQUITIES.

Rome, Oct. 28.—There has just been found upon the Appenine Way an ancient sun-dial, drawn upon marble, with the names of the winds in Greek. It is exactly calculated for the latitude of Rome. According to local circumstances, it is concluded to be the discus belonging to Herodius Atticus, and described by Vitruvius.

JERUSALEM.

We understand that an artist of acknowledged respectability and talent is now occupied with an undertaking eminently calculated to assist the antiquary in his researches upon the subject of biblical antiquities; namely, a picture of the site of Ancient Jerusalem and the surrounding country, as now seen from the Mount of Olives. The picture is to be submitted to public view for a short time preparatory to the publication of a series of plates, accompanied by appropriate descriptions.

The painting exceeds 100 feet in length, and its elevation is 18 feet.

The pipes of the engines used in France for extinguishing fire are made of flax, and are found to answer the purpose much better than those made of leather. They are woven in the same manner as the wicks of patent lamps, and can be made of any length without a seam or joining. When the water runs a short time through the pipes the flax swells and no water escapes, though the pressure be very great. They are more portable, not so liable to be out of repair, and do not cost by one half so much as the leather ones used in this country. This article is now manufactured in Glasgow.

To Mr. Tilloch.

Sir,—Allow me to call the attention of your readers to the consideration of the following singular property which I have lately discovered is possessed by that useful substance *sulphur*.

If a stick of sulphur is grasped in the hand, numerous snappings are heard, exactly like (though not so loud as) the discharge of the Leyden jar; this continues frequently as long as it is held in the hand: sometimes indeed after a short interval it ceases, but the crackling may be again produced by warming it.

Now, as we know this substance is one of the most perfect negative electrics, are we to infer that the bare holding it in the hand is sufficient to excite it, and to produce the same effects as are obtained from a glass tube, or other electric, by friction? It is clear that the noise is occasioned by the communication of a certain portion of its electricity to the hand, as it is never heard when approached by another electric body;—a piece of metal warmed, which is an equally good conductor with the hand, also produces the same sound. I have no doubt but that in the dark this noise would be accompanied by a scintillation similar to that produced by the approach of a non-electric body to an excited glass tube. As it is generally believed that holding a piece of sulphur in the hand is a cure for the cramp (and I am given to understand by a friend who has tried it, that it has that property); are we to attribute its efficacy in this instance, to the power it possesses of imparting a certain quantity of electricity to the hand, which, by immediately acting on the complaint, at once alleviates and removes it?

Any information on this curious subject will be gladly received by

Your constant reader,

Jan. 22, 1816. J. F.

QUERIES.

To Mr. Tilloch.

Sir,—I shall consider it a favour, if you will publish in your valuable Magazine the following queries and observations: they may lead to some information, which will be as acceptable to many of your readers, as to your constant student,

Veri Investigator.

The principle of bevelled wheels was, I believe, first published by De La Hire in the ſixth volume of *Mémoires de l' Académ. Paris,* 1666 to 1699. But I would gladly be informed *where* and *when* they were first put in action?

What is the *date,* and by whom were governors or lift-tenters first applied to corn-mills driven by wind? and *where* was the first governor applied to the steam-engine?

The Swedes contend with us for the priority of invention of the thrashing-machine. Can you inform me, whether they have any just ground for such a claim? If they have not, I imagine that Mr. Sterling of Perthshire was the inventor about 1758.

Has there ever been published any separate treatise upon the steam-engine? I suspect that much may have occasionally appeared in some provincial towns, near which those valuable machines are manufactured, or used to a great extent, without ever reaching the metropolis.

Medical School of St. Thomas's and Guy's Hospitals.—The Spring Courses of Lectures at these adjoining Hospitals will commence the beginning of February, as follows; viz.

At St. Thomas's.—Anatomy and Operations of Surgery, by Mr. Astley Cooper and Mr. H. Cline.—Principles and Practice of Surgery, by Mr. Astley Cooper.

At Guy's.—Practice of Medicine, by Dr. Babington and Dr. Curry.—Chemistry, by Dr. Babington, Dr. Marcet and Mr. Allen.—Experimental Philosophy, by Mr. Allen.—Theory of Medicine, and Materia Medica, by Dr. Curry and Dr. Cholmeley.—Midwifery, and Diseases of Women and Children, by Dr. Haighton.—Physiology, or Laws of the Animal Œconomy, by Dr. Haighton.—Structure and Diseases of the Teeth, by Mr. Fox.

N. B. These several Lectures are so arranged, that no two of them interfere in the hours of attendance; and the whole is calculated to form a Complete Course of Medical and Chirurgical Instruction. Terms and other particulars may be learnt from Mr. Stocker, Apothecary to Guy's Hospital.

LIST OF PATENTS FOR NEW INVENTIONS.

To George Morton, of Covent Garden, in the county of Middlesex, for his mode of attaching horses to waggons and all other four-wheeled carriages.—To enrol 14th November 1815.—6 months.

To Joseph Baader, Doctor of Medicine, Knight of the kingdom of Bavaria, for his improved plan of constructing rail-roads, and carriages to be used on such improved rail-roads, for the more easy, convenient, and expeditious conveyance of all sorts of goods, wares, merchandize, persons, and all other articles usually, or at any time, removed in carriages of any construction whatever.—14th November.—6 months.

To George Austin, of Wottonunderedge, in the county of Gloucester, and John Dutton the younger, of Hillsley, in the parish of Hawksbury, in the said county, clothier, for certain improvements in the operation of fulling woollen cloth, and for improvements in fulling-mills.—23d November.—2 months.

To Allan Taylor, in the parish of Barking, in the county of Essex, Daniel Gallafent senior, and Daniel Gallafent junior, in the parish of Braintree, in the county of Essex, for their engine for raising cold and hot water.—25th November 1815.—2 months.

To George Young, of Thames-street, London, for his improved method of making a peculiar species of canvass, which may be used more advantageously for military and other purposes than the canvass now in use.—5th December 1815.—2 months.

To Jean Frederick, Marquis de Chabanus, of Russell-place, in the county of Middlesex, for a method or methods for conducting the air and regulating the temperature in houses and other buildings, and warming and cooling either air or liquids in a more expeditious and less expensive manner than hath hitherto been done within this kingdom. 5th December.—2 months.

To James Lee, of Old Ford, in the county of Middlesex, for certain improvements in the methods before invented by him of preparing hemp and flax for their various uses, and by which also other vegetable substances may be rendered applicable to many of the purposes for which hemp and flax are used.—5th December.—15 months.

To Christopher Dihl, of Frith-street, Soho, Middlesex (in consequence of a communication made to him by a certain foreigner residing abroad), for certain improvements in the method or apparatus for distillation.—5th December.—6 months.

To John Mazel, of Poland-street, in the county of Middlesex, for his instrument or instruments, machine or machines, for the improvement of all musical performances, which he denominates a metronome or musical time-keeper.—5th Dec.—6 months.

To Samuel Clegg, of the Gas-works, Peter-Street, Westminister, in the county of Middlesex, engineer, for his improved gas apparatus.—9th Dec.—6 months.

To Robert Kinder, of Liverpool, Lancaster, for his improved method or means of propelling ships' boats and other vessels.—19th Dec.—2 months.

To Robert Dickinson, of Great Queen-Street, Lincoln's Inn Fields, in the county of Middlesex, for his improvement or improvements in the hoops or hooping of barrels.—19th Dec.—2 months.

To William Plenty, of Newbury, Berks, iron-founder, for his plough or agricultural implement made on a new or improved principle and answering a two-fold purpose, so that land or ground may be thereby both pared and ploughed.—22d Dec.—2 months.

To William Adamson, of the parish of St. George, Hanover-Square, London, for his discovered principle by which an horizontal wheel may be so moved about its axis by water, as to give it a power considerably greater than can be obtained by the application of water to a wheel in any other position.—22d Dec. —2 months.

To Joseph Reynolds, of Kitley, in the county of Salop, for certain improvements in the construction of wheel-carriages, and of ploughs, and other implements used in husbandry, to be moved by steam, heated air, or vapour.—9th January 1816.—2 months.

To Edward Cooper, of the parish of St. Mary, Newington Butts, for a method of painting paper for paper hanging.—10th Jan.—6 months.

To Thomas Deakin, of Ludgate Hill, London, and John Richard Haynes, of St. John's Street, for an improved stove-grate or fire-place.—15th Jan.—6 months.

To James Barron, of Wells-Street, Oxford-Street, for an improvement or improvements on castors.—23d Jan.—2 months.

METEOROLOGY.

The following Register of the Rain-gauge kept in Edinburgh by Mr. Adie, optician, shows the quantity of rain that fell during more than eleven years, in inches and decimals of an inch; and it appears that there was more than an average quantity fell in 1810.

Months.	1795.	1796.	1797.	1798.	1799.	1800.	1801.	1802.	1803.	1804.	Average of each Month.	1810.	1811.
Jan.	2·73	3·28	1·32	1·80	·39	2·98	1·75	·71	·80	3·72	1·998	2·280	·89
Feb.	2·87	1·40	·67	·55	1·57	·49	1·44	1·87	1·56	·57	1.399	·984	3·30
Mar.	1·37	·43	1·20	1·59	·47	1·34	·89	·50	·74	2·58	1·106	2·843	1.37
April	2·11	1·09	1·47	1·56	2·15	2·05	·60	·73	1·16	2·04	1·496	1·584	1·72
May	1·20	1·43	1·96	1·62	3·27	2·70	1·95	·86	1·13	1·58	1·734	1·435	3·69
June	2·92	1·03	2·18	2·53	·87	·52	·20	2·21	1·35	1·34	1·614	1·479	5·68
July	2·52	2·77	5·19	2·10	2·60	·40	5·25	4.15	·86	1·80	2·774	3·134	2 77
Aust.	3·62	·45	4·50	2·99	5·66	1·26	·88	2·12	2·00	3·91	2·740	3·100	
Sept.	1·12	2·21	2·99	2·28	4·02	2·53	2·66	2·37	1·82	·54	2·254	·457	
Oct.	4·87	1·19	3·24	2·15	1·99	3·38	1.59	2·43	1·00	2·37	2·416	1·570	
Nov.	4·58	1·31	1·20	2·07	1·79	·98	1·06	2·09	2·26	1·91	1·926	4·430	
Dec.	3·81	1·06	1·26	1·41	1·25	2·91	2.17	1·02	1·13	1·96	1·796	2·340	
Total	35·72	17·65	27·18	22·58	26·51	21·30	20·41	21·20	15·81	24·37	23·270	25·636	

Telford's Report on the Means of supplying Edinburgh with Water, page 5.

METEOROLOGICAL TABLE,

BY MR. CARY, OF THE STRAND,

For January 1816.

Days of Month.		Thermometer.			Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
		8 o'Clock, Morning.	Noon.	11 o'Clock Night.			
Dec.	27	42	35	42	29·20	0	Rain and Snow
	28	32	35	29	·79	0	Rain and Snow
	29	45	46	46	·82	7	Fair
	30	43	40	28	30·50	10	Fair
	31	28	30	27	·50	7	Fair
Jan.	1	28	33	26	·51	6	Fair
	2	27	34	27	·16	7	Fair
	3	37	40	35	·25	10	Fair
	4	29	42	35	·23	12	Fair
	5	36	43	40	·10	6	Fair
	6	45	48	35	29·76	6	Cloudy
	7	40	40	36	·85	8	Fair
	8	40	49	45	.50	0	Rain
	9	45	47	46	·52	10	Fair
	10	49	51	48	·48	14	Fair
	11	52	50	44	·01	10	Fair
	12	38	45	39	·10	0	Rain
	13	39	44	38	28·97	5	Cloudy
	14	39	43	40	29·21	7	Fair
	15	38	45	33	·20	0	Rain
	16	37	42	40	·67	14	Fair
	17	40	42	36	·68	9	Fair
	18	34	42	34	·69	9	Fair
	19	33	42	39	·68	4	Cloudy
	20	35	40	39	·50	5	Fair
	21	38	40	39	·20	0	Rain
	22	36	39	39	·22	6	Fair
	23	38	42	38	·12	0	Rain
	24	39	40	39	·10	0	Cloudy
	25	38	40	36	·11	0	Foggy
	26	34	40	35	·32	7	Cloudy

N. B. The Barometer's height is taken at one o'clock.

XVII. *On Aërial Navigation.* *By* Sir GEORGE CAYLEY, *Bart.*

To Mr. Tilloch.

SIR, — I AM glad to find that the public attention is called to aërial navigation by Mr. Evans, in your Magazine for November last. This subject is of great importance to mankind, and is worthy of more attention than is bestowed upon it. An uninterrupted navigable ocean, that comes to the threshold of every man's door, ought not to be neglected as a source of human gratification and advantage. Mr. Evans proposes the action of a large inclined plane suspended below a common balloon, as the means of making it take an oblique course in its ascent, and by means of the same plane to make the weight of the apparatus cause an oblique descent towards the same point of steerage. This principle is unquestionably capable of performing what that gentleman proposes, although the construction he has given is only adapted to effect the purpose in ascending, as the want of weight in the plane would prevent it from operating, excepting in a very limited degree, in the descent. In the small balloons used by Mr. Evans, the plane was, in fact, the whole burthen supported; and hence, under the small velocity generated, the descent was as oblique as the ascent. But in the balloon 80 feet in diameter described by that gentleman, the plane in ascending would receive, according to his estimate, above 2000 pounds of resistance from the air, and thus become efficient; whereas in descending it could only sustain a resistance equal to its weight, which of course will be as little as possible, and hence it will be nearly inefficient. The general principle, however, is perfectly true; and when applied advantageously, although it is an indirect way of gaining the proposed horizontal point, yet it will be as effectual as the process of tacking in ordinary navigation. Mr. Evans estimates that a Montgolfier balloon of 80 feet in diameter, with a plane suspended under it in an angle of 70° with a perpendicular line, the dimensions of which are as 1·4 to 1, compared with the great circle of the balloon, will be carried through the air by a power of ascent equal to 2792 pounds with a velocity of 28 feet per second, and hence that the travelling horizontal speed will be about 19 miles per hour.—If the resistance of air be taken at one pound per square foot at a velocity of 23 feet per second, which is two feet more than the common engineering estimate, the resistance of a globe to its great circle, according to Mr. Robins's experiments, as 1 to 2·27, and the resistance of the plane as the square of the sine of the angle of incidence,—it will require about 12450 pounds to produce this rate of conveyance in Mr. Evans's balloon; and hence the power he allows is not one-fourth part of what is required. This apparatus would, however, according to my calculation, travel with the speed of about 8¾ miles per hour*, which is quite sufficient to show the utility of the principle, whether the mistake be in Mr. Evans's figures or my own.

A few years ago I made many experiments upon the power of inclined planes, some of which exceeded 300 square feet in area: an account of these may be seen in Mr. Nicholson's Journal for November 1809, and February and March 1810. It may be affirmed with confidence from these experiments, that in obliquely descending the efficacy and steerage of the inclined plane have been completely ascertained. My object was to leave out the unwieldy bulk of balloons altogether, and to make use of the inclined plane propelled by a light first mover. Although my attention has hitherto been diverted from making further experiments, I am fully convinced that this mode of aërial navigation is practicable, and will, ere long, be accomplished. In the mean time I shall be glad to promote any promising experiments upon the steerage of balloons, and therefore offer the following observations through the medium of your Magazine.

In considering the means for obviating the relative resistance of balloons in passing swiftly through the air, the leading general principle is evidently to increase their dimensions far beyond the limits hitherto adopted. The weight of their superficial materials, and the resistance they meet with, being as the squares of their diameters; whereas their power of support being as the cubes of these diameters, it follows that their power may be made to bear any required proportion to their resistance. Thus a balloon of one yard in diameter meets with ten times more resistance, in proportion to its power, than a balloon of ten yards in diameter.

The next consideration is, that a globe is by no means the best shape for obviating resistance; a greater extension in the line of its path, with a corresponding diminution in the section perpendicular to it, may be adopted with great advantage. Keeping in view these two leading principles, the former of which places the proper scale of experiments beyond the expense that individuals choose to appropriate to such purposes, I propose that the following plan be adopted by those desirous of promoting this noble art:—First, that a subscription be entered into for obtaining a proper fund;—and secondly, that a committee be appointed by the subscribers for the purpose of carrying such experiments as may appear eligible into effect. To such a committee I should be glad to submit the propriety of making the following experiment, which would be capable of trying all the expedients hitherto proposed for steering balloons. The principle proposed by Mr. Evans is only applicable, at present, to fire-balloons; and as these are the cheapest, and upon a large scale with proper precautions may be made safe with respect to fire, I propose that the experiments be made upon the Montgolfier balloon. The scale I propose to adopt, though as small as is compatible with the object in view, will appear to any one who has not calculated the proportions required for the success of the experiment, of stupendous magnitude.—Amazement would have been the consequence of presenting to the imagination of an ancient Briton the idea of a British hundred-gun ship, when only contemplating the principles of navigation exhibited in his humble coracle covered with a skin. From the truck of the flag-staff to the extreme of the bowsprit, a vessel of this sort will measure about 90 yards; and it is a wonderful effort of human ingenuity arising from the gradually accumulating knowledge of ages:—but the *long boat* of aërial navigation commences about the bulk where the man-of-war of common navigation has reached its full growth; and what may be the vessels, *I hope not men of war*, of this sort which a thousand years of human invention may bring to light, I am at a loss to contemplate.

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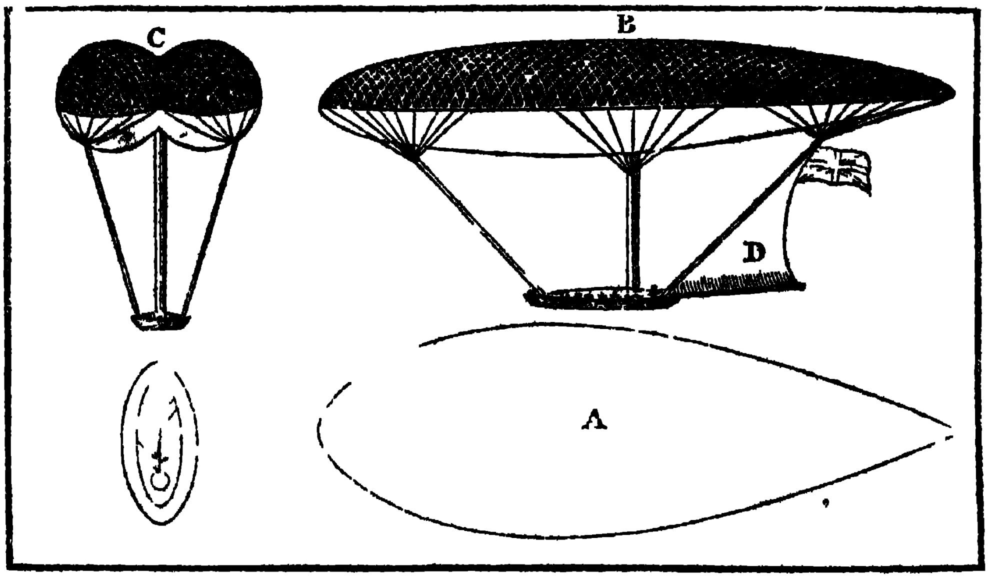
* An error in the paper of Mr. Evans was corrected in our number for December, making the velocity 10½ miles per hour.



Let A B C represent the plan, side and end elevation of a balloon or aërial vessel, made of woollen cloth, and kept to its shape by light poles attached to it, and internal cross-bracings of wire or cord, opposing the tendency to become circular from the internal pressure of the heated air: this vessel to be 15 yards in elevation, 30 in width, and 100 in length. About 27 yards below this vessel must be suspended a convenient boat-shaped car, by six ropes collecting the cordage of the netting. This boat must be furnished with a light fire-grate; and an oval chimney of thin metal must descend from the balloon, and cover the fire. This chimney to be furnished with three fine wire nets to prevent sparks from passing up it. A sail or rudder D must be attached to the boat from behind, which can be turned to either side by bracing the boom to which it is fixed. The cloth made use of being woollen will not be subject to take fire; but it is requisite that it should be made air-tight, and likewise impervious to rain, by some coats of paint or varnish on the outside. The machine being thus completed so far as it is necessary to try the principle of the *inclined plane*, as soon as the balloon is inflated, let the front ropes be lengthened and the hinder ones shortened, till it stands in an angle of about 30° with the horizon, when it will be found to rise in an angle of about 15°, and the horizontal velocity towards its destined *harbour* will be about 20 miles per hour *.—The power of the heated air would be about 17600 pounds: of this about 6800 pounds would be required to generate the velocity specified, and the remainder will be consumed in the weight of materials, fuel, passengers, &c.

It may seem at the first view extraordinary that I should propose to make use of so long a chimney; and this requires some explanation. The exterior resistance of the air to the anterior portion of the balloon, will amount on some parts of it to about 26 pounds per square yard at the proposed speed; and hence an internal pressure at least equal to this must be created, for the purpose of preserving the form of the balloon. This is most readily effected by force of the long column of heated air passing up such a chimney as I have described; for, were the air within it no hotter than the general temperature of the balloon, the 27 yards of chimney added to half the height of the balloon would create a pressure rather exceeding what is required; and I conceive it will, from its greater rarefaction, create as much pressure as will permit the prow of the balloon to be depressed to an angle of 30° with the horizon, and still enable it to resist the impression of the air in descending. It is scarcely necessary to observe that the balloon must be, as usual, furnished with a large valve at the top, and likewise with one at the bottom, to permit the escape of the hottest or coldest air as required. If the specific heat of air be to that of water as 1·79 to 1, it will require about 880 pounds of fuel to inflate this balloon, exclusive of what will be consumed to supply the waste of heat during the operation; and when a second rise is required, by having suffered the escape of air equal to the power of 6800 pounds, it will require the rapid combustion of 340 pounds of shavings, chopped straw, &c. to create a renewed ascent. Hence, including waste, probably about 100 pounds of fuel will be expended for every mile of conveyance, exclusive of the first inflation.

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* This calculation is grounded upon the following data:—That in Montgolfier balloons one cubic yard of space has been found to give 11 ounces of power—that the form of the vessel will prevent it from receiving more than a third part of the resistance that its greatest cross section would receive at the same velocity; and that a velocity of twenty-three feet per second in air, creates a resistance of one pound per square foot, according to some careful experiments of my own upon a very large scale.

When this experiment has been made, it will be easy to try whether the balloon can be driven directly forward by sails wafted by the steam-engine at a less expense of fuel. In the former case the balloon had to proceed along two sides of a right-angled triangle only to gain the length of the hypothenuse: hence, as the resistance varies as the square of the velocity, the same horizontal speed of conveyance will be obtained with rather less than half the resistance in the one case than the other. The consumption of a steam horse power is about 30 pounds of water and six or seven pounds of fuel per hour. I have made several calculations relative to this subject, but it will occupy too much space for any one number of your valuable Magazine to detail them; I shall therefore close this paper, already perhaps too long, by stating, that if the dread of fire should deter any one from wishing to promote this experiment, notwithstanding the adoption of woollen cloth to prevent it, a perfect security from this accident may be obtained by using steam in lieu of heated air for inflating the balloon, or at least a great mixture of it with the heated air. The power of steam is greater than air at the usual temperature in Montgolfier balloons in the ratio of 18 to 11, although the first inflation will cost more fuel in the ratio of 2·6 to 1. The resistance to a steam-balloon will be only as 1 to 1·38, when compared with one of the same power inflated by heated air; and hence a considerable saving of power would be the result of adopting it. But several inconveniences arise upon the introduction of steam into balloons, the chief of which are the necessity of doubling the structure, so as to suspend the steam balloon within one of heated air or gas, and of the materials being incapable of absorbing water. However, I think it very possible that the following lines of Dr. Darwin may eventually be realized:

> "Soon shall thy arm, unconquer'd steam! afar
> Drag the slow barge or drive the rapid car;
> Or on wide waving wings expanded bear
> The flying chariot through the fields of air.
> Fair crews triumphant, leaning from above,
> Shall wave their fluttering kerchiefs as they move;
> Or warrior bands alarm the gaping crowd,
> And armies shrink beneath the shadowy cloud."

I remain, sir, your obedient servant,

Brompton, near Malton, Yorkshire,
Dec. 24, 1815.

George Cayley.

XVIII. *An Attempt to draw a Parallel between the Arts of Painting and Sculpture*[*].

The appellation of Sister Arts is generally given to Painting and Sculpture: this mode of expression taken in a general sense is correct; but the relations and resemblances which they respectively present to the eyes of a common observer are very remote, when they are considered attentively and with the eyes of an artist.

Several eminent artists and amateurs, whom I have the honour to rank among my friends, having expressed their surprise that very little had been written upon sculpture, while the most petty scribbler thinks himself qualified to decide dogmatically on the merit of painters, and to dwell pedantically on all the parts of a picture; it occurred to me that the reasons for such a seeming neglect of the former art, and such an excess of criticism on the latter, were to be sought for in the essence of the two arts. In these few pages I shall therefore endeavour to give a general idea of both.

Painting strikes the senses most forcibly, and the aid of colours gives it the advantage of closely resembling nature. Not only do the abundance and *éclat* of its productions diffuse it more widely and facilitate its reception in the world; but the means of its execution are so familiar and so well known, that all men regard it as a common property. But it is not in this point of view, nor in this spirit, that it is made for all the world.

Sculpture, more confined to the workshop, less in view and of more difficult removal, slower in its operations and less extensive in its field of composition, not only curtails, and as it were shuts up, but obscures the career which is always open with respect to painting, against narrow minds and professed boasters.

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[*] Translated from an unpublished manuscript of Count Caylus, in the possession of M. Fayolle, member of the French Institute.—*Magasin Encyclopédique.*

The facility of speaking and the silence above alluded to are, therefore, wholly dependent on the nature of each of these arts. I am the more anxious to communicate these ideas on painting and sculpture, because those who practise them evince no desire to know that the labour and execution, the methods of which as transmitted from master to scholar are adopted in the school, to say the truth, are a kind of routine which may have advantages for men of middling talents, whose habitudes induce idleness. But this routine and this beaten track are not only enemies to greatness, but lead artists to despise theory, and reject the reflections which men of letters may chance to communicate. This prejudice, the source of the greatest misfortunes to the arts, is absolutely false in its principle.

The slightest reflection teaches us that the theory of an art always serves to the perfection of its practice; and artists themselves prove every day that these parts are inseparable: in fact, those of superior talents would not retain the pre-eminence which they enjoy, without the application of the essential parts of theory to the beauty of the touch of the chisel, the perfection of the tracings, and general grandeur of idea. I shall admit that it may not be requisite that they should reason methodically on their art, or write upon it with order: all this may not be required of them; but they ought to be able to speak of it correctly, and with decent warmth: in short, they ought to be luminous. Could they have these advantages, if this theory which they despise were not indelibly but unconsciously rooted in their minds? It does exist there certainly, but mixed and confounded with what they call their practice. We may be the more assured that the theory is nothing but the *rationale* of the art, because artists continually use it without having the least doubt of its potency: in fact, they both oppose it incessantly to the critiques which are made on their productions, and use it in those which they make themselves on the works of ancient or modern artists.

In order that sculpture may be brought within the comprehension of persons in general, who have no idea of the resources of intellect and dexterity of workmanship requisite for its operations, I think it best to compare all its parts with those of painting; to point out the portions which are common to both, and which establish their sisterhood; and, finally, to notice the facilities or difficulties which both kinds of artists have to encounter. This arrangement appears to me to be the simplest, and the most capable of making known the real difference of the two arts.

Their common parent is Genius, that portion of the divinity impossible to be misunderstood and more impossible to define, which strikes, produces, illuminates, creates, and cannot be confounded with any part of the human mind, which is always little in its presence. This universal agent hovers equally over the two professions, and diffuses, more or less, sparks of fire from his luminous centre:—happy are those artists who are sufficiently prompt to receive them, and sufficiently skilful to make the proper application.

The imitation of nature is constantly the essence, the basis, the property, and finally the common mother of the two arts: but imitation alone produces mere pieces of ice; it is but a slave, if genius refuses to accompany it: it is genius which furnishes the grand means which lead to expression—to genius we owe that penetration into the object, and that just and accurate transport with which the soul is affected, and which flows from the representation of action itself.

Painting and sculpture equally require the influence of a father and a mother, to whom they owe the existence, or rather the reproduction, of every instant of their being. Nevertheless, far from sharing their gifts to their favourite daughters by the same ways, genius and imitation subject them not only to their particular object, but to the means which may exalt the magnificence of their sources, and beget an admiration which ought to be ascribed to its principle. Another object still more worthy of these divine essences, is that of transmitting to posterity great examples of heroism and virtue. Men who are truly wise and truly heroic have, perhaps, no desire to be encouraged by the honourable recompenses which the arts distribute; but those who succeed them may be guided and warmed by this reward of good actions. Thus we may assert, that the monuments of antiquity have often been the source of the greatest virtues; for it is evident that living or too recent examples do not make the same impression as the works of sculpture dedicated by antiquity to virtue and glory:—the latter present an isolated example purified from every defect which could obscure it; the former, *i. e.* the models of living or too recent characters, lose all their *éclat*; while their value is diminished by the self-love of men who do not like to be surpassed by their contemporaries, and who receive those personal impressions, those national ideas, and finally imbibe all those prejudices, which the mind shuts out with the utmost difficulty as a consequence of these truths. The arts ought to draw from antiquity the examples necessary for humanity.

I trust that this digression will be forgiven, as I am here inquiring into the merits of an art which men in all ages have charged to transmit to posterity just ideas of the grandeur of the virtues and the strength of empires.—I now return to the details which I have undertaken.

Painting possesses ample means of making itself undertood: the aid of local and aërial perspective, the multiplicity of plans, aptness of site, facility of accessories, the instant of every movement, all real and imaginary positions, and finally the colour of the objects represented. What are the limits of all these advantages when they are guided by the hand of genius? This inquiry does not belong to this place. The above enmeration is merely given, that all the privations under which sculpture labours may be amply perceived.

Sculpture can only represent figures holding firmly by the ground, from being obliged to have recourse almost always to supports capable of sustaining the limbs and enabling them to resist the weight of the body. What I say on this occasion regards single figures only, and which are most usually treated by sculpture. It is true that groups are sometimes exhibited, but in general the number of figures of which they are composed is small; for it is rare to find them more extensive than that of Dircé, that which we call the *Farnese Bull*, or the *Baths of Apollo* at Versailles. Bas-reliefs furnish sculpture with several of the parts which painting uses at pleasure: they afford the means of representing surrounding scenery, landscape, multiplications of plans, and consequently of perspective The bas-relief has besides the advantage of giving an idea of the kind of light and the point of view for which the sculptor has composed his piece. I shall add that, like the painter, he has only a surface to adorn, that he may express actions varied at pleasure, and that he may arrange at will all the positions of nature and fiction. Notwithstanding this increase of succours, each art being retained within the limits which its essence prescribes to it, these kinds of work are in several respects always inferior to painting, and bas-reliefs often leave something to desire, even without comparing them to pictures; for it is certain that they are attended with great difficulties, and we have not always occasion to remark, on viewing them, that the subject is well understood from the bas-relief. Their execution demands, therefore, a peculiar sort of talent.

After having overcome the difficulty of supporting the figure, *i. e.* having found a method of doing so without making the action appear constrained or unnatural, sculpture is obliged to discover a position which shall be happy in every respect: and whoever has not reflected upon this difficulty, cannot conceive the pains necessary to be taken to find an accurate and agreeable adjustment of all the parts and for every point of view. We ought not to forget that painting works under a single point of view.

The parts which are most isolated, and particularly the arms, do not less embarrass the sculptor: he ought to have support and solidity constantly in his mind; they are of such necessity in execution, that in order to attain them, he is frequently compelled to renounce the most expressive attitudes. How great is the constraint therefore in composition! What are the curtailments to which genius must not submit!

The accessories concur to the action, and serve to make it known; their assistance is a very great resource to the arts: eloquence even makes use of them, for an epithet appears to me to be an accessory, or if we please an attribute: the painter's only fear is lest he should abuse them; whereas the sculptor, restrained within very narrow bounds, can employ only attributes often of very general application; since they sometimes accord with several figures, and since he can only place them on the clothing, on the head, on the hands, and sometimes on the support which has been mentioned.

I do not speak of the convenience of positions, with respect to age and character, nor of the correctness of the tracing, of the details of putting the limbs and other organs in the right place, for these parts are as necessary to sculpture as to painting: but the latter, having only one front or one point of view to represent, is executed with the more facility, because, if the painter perceives some error, or some degree of perfection which he can add, he is master of the art of instantly effacing and retouching: the sculptor, on the contrary, is deprived of this advantage; he cannot retrace his steps from the moment he has chipped off a piece of marble. I shall pass over in silence the constraints which the dimensions of the block frequently occasion in producing the given figure. But these constraints are not all which the art undergoes. The painter chooses the light which he wishes to fall on a surface: the sculptor has no choice; he has all kinds of light at once, and this very abundance is to him a source of embarrassment; for he is obliged to consider and to reflect on all the parts of his figure, and to work them accordingly: it is himself in fact who, as it were, gives the lights to his subject. A sculptor must also make several sketches until he finds the proper attitude: in this respect the mind of the sculptor is more creative than that of the painter. But this vanity is not satisfied but at the expense of much reflection and fatigue; whereas the painter has all the oppositions of colour, accidents and effects of every kind at his command, to produce correspondence and harmony in parts which concur most to general harmony, *i. e.* to the charms of sight. The sculptor has no such advantages. He has no guide but experience, and, if we may so express ourselves, the fountain of his chisel, as the painter has that of his pencil. The imitation of the human skin is the object and principle of the imitative arts: the flesh merits the particular attention of both arts, since there is no part more prominent and with which the spectator is more struck: it ought to be perceived chiefly over the muscles which it covers, without diminishing their play or size, and without altering their strength or position. But how great are the differences in the method of expressing this skin! Sculptors must search for examples in the *chefs-d'œuvre* of the Greeks; they alone have given the model of profound knowledge and sublime execution: they must place all their confidence in the correctness and beauty of their works, and must not seek to surprise into admiration by a contrast in the positions. It would have been desirable if ancient authors had made any mention of their method of studying. It is evident, however, that it must have been different from that of the present day; for even those among the moderns who have most copied and who have been most penetrated with admiration of the Greek sculptors, have never caught their style nor method of working. We merely know from the accounts of Pliny, that, far from neglecting the theory, they reflected deeply on their art. The great number of artists whom he mentions as having written profoundly on this subject, does not admit of our refusing to believe this compliment.

I return to the means which the painter possesses of giving expression to the skin. It is easy for him to colour his work; and, without speaking of half-tints, to support it by large shades. He may easily enliven it by forcible touches which produce an agreeable opposition and a happy contrast with this same skin; whereas the sculptor cannot produce the same effect without the greatest pains, and he does so without the aid of any of those chance-touches of the painter which resemble the sallies of the imagination in a *bon mot* which reflection never could have foreseen. When the studies of the painter are at an end, he may sit by his fire and survey all the parts of his picture: he covers, extinguishes, revives, and harmonizes: the sculptor with a sharp chisel, which acts only when it is struck with a mallet*, and without having it in his power to restore the substance which it has removed, cannot give harmony to his figure without the most laborious attention and the most accurate judgement, subject to the idea of a perfect whole, which he ought to have constantly present in his mind, without having it in his power to abandon it for a moment. From the hair of the head to the sole of the foot, how many touches of the chisel does it not require to render a piece of sculpture sublime!

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* All the fine works of sculpture are finished with the point of the chisel: the rasp is a most pernicious instrument.

The two arts cannot pay too much attention to the subject of draperies. We might make a long dissertation on the abuses in this department: but sculpture requires in this respect still greater pains than painting. Bernini, whose talents are unquestionable, has given much amplitude and flow to his draperies. This novelty has been productive of bad consequences. In order to avoid the difficulties of forming the naked figure, an excess of drapery has been resorted to by the followers of Bernini: they have forgotten that the clothing ought always to recall the idea of the form of the parts under it; latterly, in Italy as well as in France, statues have been covered with a multiplicity of large folds, and have exhibited motions which nature never dictated. Thus stepping from abuse to abuse, the draperies have been composed separately with great nicety, and frequently directly against the natural effect of the weight and movement of the figure which they are intended to clothe. This is not astonishing, since they are intended to be fixed upon a lay-figure, and adjusted at pleasure. The ancients are still our masters in this respect: it was not through ignorance or idleness that they made choice of light draperies, which have been erroneously denominated *moulded draperies;* but the dresses which they represented being composed of gauze or muslin, preserved the naked shape, and conveyed the idea of all the movements of the body in a manner so agreeable, that the eye was always satisfied, and nature gained in some respects from voluptuousness.

It is scarcely necessary to say that this proposition cannot be general; and that decency requiring a great degree of modesty, the figures which belong to religion in particular cannot admit of much nakedness. But would it be impossible to dress them in thinner clothes, less ample, and less loaded with great folds? As to those which we borrow from fable, we have too many reasons for following the examples which the ancients have given us.

It cannot be said in favour of the sculptor that he has the advantage of moulding the parts. Without entering into a detail by which painting may equally profit, it would result from this operation that all statues would be excellent, because there is no part of the human body which cannot be moulded and copied by the compass; nevertheless the contrary is too well demonstrated: in fact, not only does nature sometimes present imperfections, but all the parts selected and moulded do not agree with each other. The joinings at least must be supplied: besides, the character of a moulded part is generally frigid, and does not accord with the subject intended to be represented: and lastly, the proportions are different. It must be admitted, however, that this facility of moulding has frequently been used by ignorant sculptors; but it will always be easy to distinguish the use which they have made of it: nature breaks out, and the eye is struck with it the moment it is perceived. Thus we may say in general, that by supposing an equal degree of mediocrity, the sculptor with the aid of the mould will appear superior to the painter, when both of them work without genius.

It has been generally thought that real colours applied to a work of sculpture ought to produce the most perfect imitation. This practice, adopted in the barbarous times of antiquity, was preserved throughout Europe until the revival of the arts. We find even at this moment in country towns statues of saints plastered over with various colours. The grosser senses of the peasantry are struck with this alliance of sculpture and painting; but it is the only beauty they have: for I can safely assert, that if Apelles and Lysippus could have joined their talents upon one and the same statue, they would not have produced any thing agreeable or satisfactory: the two arts which these great men have illustrated, lose equally their advantages by uniting their efforts; and nothing so forcibly proves their real difference, as the productions which arise from their copartnership.

Colours when placed on a statue present no shading: the details of the figure become fixed and immoveable; and although, physically speaking, they cannot be otherwise, the pencil and the chisel produce illusions, giving rise to ideas of the motion which suits the parts, and present several details of the passions which they have seized and taken, as it were, on the wing.

The examination of a drapery will serve as a term of comparison, and may give an idea of the way in which this operation of colouring destroys the fine and delicate expression of the passions.

I shall suppose this drapery to be loose and flowing. The painter will give to his picture either its lightness, the strength of the wind, or that of the action. The drapery of the same kind will be represented by sculpture much less extended, but it will be sufficient if it seems to float in the air. From the moment that it is coated with paint it will become heavy; its folds, which are very fine in mere sculpture, will appear loaded. The contour, deprived of the effect of opposition, such as the painter gives it at pleasure in a picture, will become heavier, as well as all the details of the mass: the salient parts treated by sculpture can produce only harsh and crude effects; for colouring cannot suit all the aspects of a lay-figure, and the work will necessarily be deprived of every kind of harmony: thenceforward, in short, it can only be regarded as an assemblage of masses, without any general or particular connexion.

Thus, each of these arts, residing within bounds prescribed by nature, ought to keep to its advantages, and surmount its defects by touches mutually borrowed. Painting, notwithstanding the grandeur and abundance of its means, always endeavours to produce the effect of reliefs, and thus to imitate sculpture. The latter, confined to solid matter, cannot be susceptible of harmony and opposition, except from the variety of the workmanship of the chisel, and by the more or less of decision in the shading: it cannot draw this harmony from any thing but from itself, or, what is absolutely the same, from the mere colour of the matter upon which it works: in order to please, it seeks the harmony of painting:—both arts, therefore, mutually lend each other assistance. In this case they are sisters; but they are different in their methods of attaining the same end, and they cannot reside under the same roof.

It results from these reflections, founded on the essence and the detail of the two arts, that the sculptor, having less assistance, seems to have more merit when he arrests and astonishes the spectator, to make him feel all the grandeur of a great action: but at the same time the spectator requires a great variety of lights to judge by; and consequently painting, being more adapted to the capacity of all men, and flattering their idleness more, must necessarily have more friends and a greater number of partisans.

XIX. *New Outlines of Chemical Philosophy.*

By Ez. Walker, *Esq. of Lynn, Norfolk.*

[Continued from vol. xlvi. p. 433.]

"*Of the Decomposition of Water*.*"

"The antiphlogistic theory has received its greatest support," says Dr. Priestley, "from the supposed discovery that water is resolvable into two principles; one that of oxygen, the base of dephlogisticated air; and the other, because it has no other *origin* than water, *hydrogen*, or that which with the addition of *calorique*, or the element of *heat*, constitutes inflammable air. 'One of the parts of the modern doctrine the most solidly established,' say M. Berthollet and the other authors of the Report on this subject (*Examination of Kirwan*, p. 17); 'is the formation, the decomposition, and recomposition of water. And how can we doubt of it, when we see that, in burning together fifteen grains of inflammable air, and eighty-five of vital air, we obtain exactly an hundred grains of water, in which, by decomposition, we find again the same principles, and in the same proportions? If we doubt of a truth established by experiments so simple and palpable, there would be nothing certain in natural philosophy.'

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* From Priestley's Essay on Phlogiston, p. 41.

"Notwithstanding the confidence thus strongly expressed by these able and experienced chemists," says Dr. Priestley, "I must take the liberty to say, that the experiments to which they allude appear to me very liable to exception, and the doctrine of phlogiston easily accounts for all that they have observed.

"Their proof that water is decomposed, and resolved into two kinds of air, is, that when steam is made to pass over red-hot iron inflammable air is produced, and the iron acquires an addition of weight, becoming what is called *finery cinder*, but what they call oxid of iron; supposing that there is lodged in it the oxygen which was one of the constituent parts of the water expended in the process, while the other part, or the hydrogen, with the addition of heat, assumed the form of inflammable air.

"But in order to prove that this addition of weight to the iron is really oxygen, they ought to be able to exhibit it in the form of dephlogisticated air, or of some other substance into which oxygen is allowed to enter; but this they have not done," &c. —Dr. Priestley on Phlogiston, p. 42.

"*An argument against the decomposition of water from the different proportions of the elements of which it is supposed to consist, according to different experiments.*

"According to the new theory, water consists of two principles, oxygen and hydrogen; and they are separated by iron, or charcoal, in a red heat, uniting with one of them, and suffering the other to escape; and therefore if in any case a quantity of water be wholly expended in forming air, and only one of the kinds be found, it will be that this water does not consist of two elements. Now, according to one of my experiments, water would appear to consist of only one of the kinds of air, and according to another of the other.

"I have shown that by a slow supply of water in sending steam over red-hot charcoal, the whole of the produce is inflammable air, without any mixture of fixed air, or the production of any thing, aërial, fluid, or solid, into which oxygen can be supposed to enter.

“ From this experiment, therefore, conducted in this manner, it might be concluded that water consists of hydrogen only, without any oxygen.

“ But according to my experiment with *terra ponderosa aërata*, it may be proved to consist of oxygen only. For when steam is sent over this substance in a red heat, nothing but the purest fixed air is produced; and yet the whole of any quantity of water may be expended in that production. As water is not said to contain any *carbon*, this must be supplied by the *terra ponderosa*, and all the oxygen by the water. For according to the theory fixed air consists of 28 parts of carbon, and 72 of oxygen.

“ These experiments favour my hypothesis,—that water is the basis of all kinds of air, and therefore, that without it no kind of air can be produced.”—Dr. Priestley on Phlogiston, page 51.—Northumberland, Feb. 1, 1800.

“In my opinion,” says Dr. Priestley, “and that of long standing, that black calx of iron called *finery cinder* contains no oxygen, but only water.

“Now what I maintain is, that when finery cinder is revived (which, however, is not done without the introduction of phlogiston*,) nothing but water is separated from it.

“ Northumberland, America, Nov. 5, 1802.”—Philosophical Journal, vol. iv. p. 66.

Experiments which tend to confirm the truth of Dr. Priestley's have lately been published by Sir Humphry Davy†.

“ I have mentioned, page 172,” says Sir H. Davy, “that in the electrization of a globule of mercury in water, oxygen appears to be combined with the metal, and yet no hydrogen evolved. I have made a number of experiments on this subject, and have ascertained that, in the process described, oxide is formed, without any apparent compensation, in the production of inflammable matter; nor was I able to detect any combination into which the hydrogen could have entered; so that these experiments as they now stand, would induce the belief that water is the ponderable basis of both oxygen and hydrogen, and that these two forms of matter owe their peculiar properties either to the agency of imponderable substances, or to peculiar arrangements of the particles of the same matter. But such a formidable conclusion as this must not be hastily adopted; for in all other cases oxygen and hydrogen appear as perfectly inconvertible substances; and in no other instance can one be procured from water without the correspondent quantity of the other, or without some product in which the other may be supposed to enter."

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* Scheele says that “ phlogiston is a true element and a simple principle.”—Scheele on Air and Fire, p. 103.

For a more particular account of phlogiston, see Philosophical Magazine, vol. xlvi. p. 430.

† From Davy's Elements of Chem. Phil. p. 485.

The French chemists have introduced innumerable errors into science, by drawing false conclusions from their experiments. When they saw that, in burning together 15 grains of inflammable air, and 85 of vital air, they obtained exactly 100 grains of water, they concluded that water is a compound of those two gases. But light and heat were produced in their experiment, as well as water: but these phænomena escaped their notice; and this oversight might lead them to form an erroneous hypothesis. Since nothing that is inflammable can be procured from pure water by any means yet discovered, it follows of course that the elements of combustion must first be put into water before those two gases can be obtained, which are supposed to be its component parts.

Now, when a Leyden phial is discharged through the air, light and heat are produced; but when it is discharged through water, two gases are formed by taking up a portion of it, which, by a certain modification, constitutes their ponderable basis. And an electric spark being passed through a mixture of these gases, combustion is produced; for, the elements contained in them and in a charged Leyden phial being the same, the effects in both cases are produced by the same cause.

From these incontestable facts it appears, that the authors of the new theory mistook the formation and decomposition of the two gases, for the decomposition and recomposition of water. And as this false hypothesis has gained so much ground in England as to be received as a chemical axiom, every correct experiment ought therefore to be brought forward, that tends to refute an erroneous opinion which retards the progress of true science.

Lynn, Jan. 26, 1816. EZEKIEL WALKER.

[To be continued.]

XX. *On Water-Wheels applied in propelling Vessels in Navigation.*

To Mr. Tilloch.

SIR, — THE practice of navigation by the rotary movement of wheels having excited the attention of public authorities, as well as of private individuals, perhaps you will allow the following suggestions to be made known to the readers of your Magazine.

Let it be supposed that the axle of a wheel is furnished with six equidistant planes projecting from the axis, at right angles to the line of its direction, in the form of a common water-wheel. If the axle be made to rest upon the surface of stagnant water, and the wheel be made to revolve quickly through the fluid; the water, which is displaced by the lower planes, will be replaced by the contiguous particles of water from the front, from below, and from either side. Three of the planes will pass through the fluid at the same time, and the motion of the wheel will be much more resisted than if each of the planes could be made to pass singly through the fluid at separate times. But, of the three planes passing through the fluid at the same time, the one which has just entered the water, and the other which is just going to emerge from the water, have both together very little tendency to impel the axle along the surface; their waste of power more than counterbalances their efficient power.

Let a wheel have its axis in the diameter of a hollow cylinder, and be made to revolve through water at the depth of the axle; the water, extruded by the planes, can only be replaced through the front passage: there can be no lateral currents; the water in front will be lowered by being drawn into the cylinder by the moving planes, and the water behind will be raised by the quantities forced out in that direction; both of which effects will tend to cause a movement of the machine along the surface of the sustaining water. In this state of a wheel, any number of planes will scarcely augment the resistance to motion, for the water will be driven through the cylinder by the advanced plane with the velocity of that plane; and the current which follows, will be caused by the lateral pressure of the fluid only: therefore the retired planes will find the requisite velocity already communicated to the water with which they first come in contact, and will move in concert with the stream, unresisted in any perceptible degree, till they, successively being brought in advance, will exercise alternately a similar function, of acting by a separate impulse against the resisting fluid. Under these circumstances, the resistance to motion will be the pressure of water upon the area of a single plane. The horizontal percussion of a descending plane against the surface of the fluid will be imperceptible, and exempt from all those inconveniences which would result from the same act of a wheel moving in free water.

Again, let three parts of a case be strongly connected together, namely, a flat bottom and two sides, and let the axle of a wheel be supported in the sides so as to revolve clear of the bottom. Let the top of the case be a curved plate drawn over the sides, air-tight, and made to descend both ways, round the perimeter of the wheel, about twenty-five degrees below the axle: two parallel openings will then remain at the lower part of the case, equal in depth to one-third the diameter of the wheel. Let each plane be one-third the diameter in depth, which size will leave the central part of the wheel to retain a considerable open space, according to the present general structure of steam-boat wheels. Let the machinery, thus formed, be lowered into water till the wheel becomes wholly covered with the fluid; and in that state, through a tube which has been prepared for the occasion, let air be forced into the machine till the water becomes depressed twenty-five degrees below the axle, when it will just meet the top of the lowest plane *. If the wheel be then made to revolve, the water will be drawn and extruded through the parallel openings at the bottom of the case, and the machine will be moved from its position by the resistance of the fluid to the acting planes. The condensed air by its specific levity will always occupy the upper sections of the case, and by its elastic pressure will keep down the inclosed water very nearly to one determined horizontal level. Passing through the open space round the axle, the air by its fluent and springy properties will continue to arrange itself in the upper sections, conformably to any motion of the wheel, and in such a manner that its elastic pressure shall act almost equally on either surface of the upper planes, without producing a sensible obstruction to their movement.

The machinery can easily be applied to any part of a boat or vessel; but it is calculated to ascertain if a central station can be practically occupied by a wheel.

To prevent the escape of the condensed air through the groove in which the working end of the axle revolves, one method shall be described. Let a box, two feet long and two inches deep, by means of circular holes in the sides, be forced upon the end of the axle at the exterior of the machine, to which it must be closely attached. Let the box be nearly filled with water or oil, and let the condensed air, from the interior of the machine, be admitted through a fixed communicating tube, into the box above the fluid. The air, which endeavours to escape round the axle by its own elastic power, will then be resisted by the same power acting upon the fluid in the external box, and by the additional weight of the fluid also: therefore the air can never escape in that direction, unless the more dense fluid be first ex-

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* Our correspondent is probably not aware that the idea of inclosing the wheel in an air-tight case, and depressing the surface of the water by means of injected air, was some years ago embraced in a patent obtained by Mr. Trevethick alone, or in conjunction with Mr. Dickinson.

pelled

pelled from the circumference of the axle which is surrounded by it in the box. Any requisite supplies of water or of oil can be furnished to the box by a variety of simple means; and thus the axle may be allowed to revolve in circles, whose concave perimeters will not even embrace it water-tight, and yet preserve the air securely from escaping.

The centre of a vessel is that part which is least affected by any agitation of the elements; and it may, perhaps, be practicable to assign a central position to the wheel; and if that assignment of place be practicable, the wheel may be made to act efficiently when the whole circumference revolves at any depth below the external surface of the water through which the vessel moves. An inclosed wheel may be comparatively small in diameter, because the descending planes strike the water horizontally without any obstruction to their progress; a waste of power will be saved, because the resistance to motion will be the pressure of water upon the area only of a single plane; and the exterior case will be an important protection to the wheels.

As an act of amusement, a strong box had its top and both ends removed, and was fastened by iron bars to the stern of a heavy row-boat at sea; a wheel was placed upon the sides of the box with its axle touching the water, and then made to revolve by pressing with the right and left hand alternately upon the perimeter. The boat was impelled half a mile through the waves; but the power was badly placed, and likewise too small to give any velocity to the wheel. The stern is the very worst position for a wheel; the water at the stern of the boat is lowered by the moving planes, and much efficient pressure is destroyed by the act.

E.

XXI. *Answer to the Geological Queries of a Constant Reader*[*].

To Mr. Tilloch.

Sir,—To the questions put to me by a correspondent, at p. 12 of your last Magazine, on the subject of the different repositories of coal in the NE part of England, I shall reply with as much brevity as the subject will admit, and hope my answers may be found satisfactory.

To query 1.—Mines of coal and quarries of encrinal limestone are worked at short intervals from the sea-coast of Northumberland N of the Coquet in a direct line to Alston Moor; but the valuable seams found in the low country have basseted out before the mountainous district is reached. The thin beds of crow coal are subordinate seams in the same formation.

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[*] See our last number, p. 12.

2.—The overlying masses and beds of basalt situated in the NE of Northumberland resemble those of the King's Park at Edinburgh, in miniature; whereas the great whin sill of the lead-mine country appears to be a wedge-shaped bed of basalt thickening towards the SW, and separating the more regular strata in the same manner as the toadstone of Derbyshire.

3.—Clay, ironstone and shale comprising muscle shells are met with from the shore at Holy Island to the banks of North and South Tyne, and in the Newcastle coal-field from the rocks at the mouth of Shields Harbour to Wylam Colliery.—See the Map of Northumberland.

4.—The calcareous red and white sandstones, shales and gypsum, over which the Tees flows from the vicinity of Croft to the sea, and which I conjecture is the red marl formation of some south country geologists, was proved to comprise thin seams of bad coal on the Dinsdale estate, by borings made in 1789; and for the fact I beg leave to refer to the sections in the port folio of the Geological Society.—This set of strata certainly rests upon the magnesian limestone, and is covered by the alum shale rock.

5.—The specimens of coal which were presented me by a gentleman who collected them on the Lincolnshire coast are black as jet, though resembling brown coal in structure. I am aware that the bituminous shale situated immediately below the hard chalk, and out of which clunch these fragments of coal are conjectured to have been washed, does not occur on the surface at Louth;—yet I cannot think the coal worked at Easingwold should be referred to the same repository; but, from the ammonites and other marine exuviæ there detected, should rather refer it to the thin coal-formation which lies upon the alum-rock, and is covered by the oolite limestone in the neighbourhood of York, &c. particularly the dip of the Whitby formation being towards the South West.

Should this supposition be erroneous, I hope your correspondent will take the trouble to set me right.

I am, sir,
Your most obedient servant,

Newcastle-upon-Tyne, Feb. 10, 1816. N.

P.S. Brown ligneous coal is met with among the basaltic rocks of the Giant's Causeway, Antrim.

XXII. *On the Depression of Mercury in the Tube of a Barometer, depending on its capillary Action.* *By M.* LAPLACE*.

It is necessary to determine the magnitude of this depression in order to render barometers comparable with each other. For this purpose we find, in the Philosophical Transactions for 1776, a table of corrections formed from experiment by Lord Charles Cavendish. At this period the theory of capillary action was unknown [?]: but this theory having been since discovered, and reduced to the fundamental principle of chemical affinities, that of a mutual action of the molecules of matter, decreasing with extreme rapidity, so as to become insensible at the smallest perceptible distances, it becomes proper to derive from this principle the table of the depression of mercury, and to borrow from observation only the necessary elements, as is usual in astronomy. In this manner we have the advantage of obtaining those elements with all possible precision, by comparing the whole of the phenomena depending on them with the results of theory; and we avoid the small irregularities which the errors of observations introduce into a table formed from experiment only. In this case, the elements required are the angle which the surface of the mercury makes with the side of the tube with which it is in contact, and the depression of the mercury below the general level, in a very narrow tube of glass. The more or less perfect dryness of the tubes may affect these elements. We know that their interior surface is lined with a stratum of water which it is very difficult to remove. It is within this stratum that the mercury of the barometer rises and falls: its thickness is sufficient to render the action of the glass on the mercury insensible; and the depression of this liquid in the barometer is determined by the reciprocal action of water and mercury. The boiling of the mercury in the tube diminishes more and more the thickness of the aqueous covering; but it appears that in the best barometers it still remains sufficient to render the action of the glass insensible. We find in the excellent siphon barometer of the observatory, that the convexity of the drop which terminates the two columns of the liquid, is not sensibly different in the two branches; and I have concluded from the experiments of M. Gay-Lussac, that this convexity is the same that takes place in a tube of glass completely moistened, provided that the water do not cover any part of the surface of the drop; " for if the whole surface is covered, I have shown that this surface then becomes that of a hemisphere." If, however, we boil the mercury for a very long time in a barometer tube, we at last so far diminish the thickness of the stratum of water, as to render sensible the action of the glass on the mercury. It has been shown by the excellent experiments of Casbois the Benedictine, that by means of a long-continued ebullition, the surface of the drop becomes less and less convex, then plane, and at last concave; and in this case the capillary phenomena change their nature, and the depression is converted into an elevation. But in making barometers, the boiling is never carried so far; and the action of the glass on the mercury not being sensible, whatever differences there may be in the materials of the glass, they have no influence of the capillary effects of the tubes. We will suppose then, agreeably to experiment, that the angle of contact of the surface of the mercury with the sides of the tube is the same as in the open air. This angle, and the depression of the mercury in very narrow tubes, are very difficult to determine by experiment; they may be collected from different phenomena, such as the thickness of a broad drop of mercury on a horizontal plane of glass; the difference of the level of the general surface of a quantity of mercury, and of the contact of this surface with the vertical side of a vessel of glass; the depression of mercury in very narrow tubes; and this same depression when the mercury is introduced into a tube of glass, so moist, that its surface is covered by a small column of water. I have inferred from the whole of these phenomena, observed with very accurate instruments by M. Gay-Lussac, that the angle of contact of the surface of the mercury with the glass is ·48 of a right angle [, or 43° 12′]; and that the mercury, in a tube of glass of the diameter of one ten-thousandth of a millimetre, would be depressed 94766 millimetres below the level, [or in a tube of a ten-thousandth of an inch, 146·9 inches].

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* From the *Connoissance des Tems*, 1812, p. 315.

The following table is deduced from these elements, according to which the action of mercury on itself is, for equal volumes, very nearly six times and one-third as great as that of mercury on water.

In order to form this table, it was necessary to integrate by approximation the differential equation of the second order belonging to the surface of mercury contained in a cylindrical tube of glass. This equation, which I have given in my Theory of capillary action, furnishes a very simple expression of the radius of curvature of the generating curve of the surface. Considering this curve, therefore, as a series of small circular arcs, described with these different radii, and joining each other at their extremities, we shall have the corresponding ordinates of the curve, so much the more precisely as we employ a greater number of divisions. The extent of the curve, beginning from the vertex, is the angle which its termination makes with the horizon, amounting in this case to 52 decimal degrees: this has been divided into twelve equal parts, and the depression of the mercury supposed successively equal to 4·5, 4, 3·5, 2, 1·5, and 1 millimetre: then for each tenth down to ·1 and ·05 millimetre. The depression being always inversely proportional to the radius of curvature at the vertex of the curve, the first radius was immediately known. This radius gave the values of the absciss and ordinate corresponding to the first division, considering it as the arc of a circle, the absciss being always taken upon the axis of the curve, beginning from its summit. These first values, being substituted in the expression for the radius of curvature, gave the second radius, and by means of this the increments of the absciss and ordinate in the second division were determined, considering the curve again in this part as a circular arc described with the second radius of curvature. In this manner the second values of the absciss and ordinate were obtained, by means of which a third radius of curvature was determined. Proceeding in the same manner to the last division, the last value of the ordinate becomes equal to the semidiameter of the tube corresponding to the supposed depression. But for a depression below eight-tenths of a millimetre, the radii of curvature near the vertex are so large, that it was necessary to divide the extent of the arc into smaller portions; the calculation has therefore been made for every two decimal degrees, as far as 12; and for the first six degrees, by the assistance of converging series, which I have derived from the differential equation of the surface of a liquid, when the greatest inclination is inconsiderable.

The formulæ and the series which have been employed for the first six decimal degrees are these. Let $V^{(r)}$ be the inclination of the end of the curve at the lower extremity of the rth division; let $z^{(r)}$ and $u^{(r)}$ be the absciss and ordinate corresponding to the same extremity; let $b^{(r)}$ also be the radius of curvature at the same point, and b at the vertex of the curve; the differential equation of the curve will give $\frac{1}{b^{(r)}} = \frac{2}{b} + 2\alpha z^{(r)} - \frac{1}{u^{(r)}} . \sin V^{(r)}$; α being a constant coefficient equal to $\frac{1}{6\cdot5}$, when the millimetre is made unity. We shall then have $u^{(r+1)} = u^{(r)} + 2b^{(r)} \sin \frac{1}{2}(V^{(r+1)} - V^{(r)}) . \cos \frac{1}{2} (V^{(r+1)} + V^{(r)})$; $z^{(r+1)} = z^{(r)} + 2b^{(r)} \sin \frac{1}{2} (V^{(r+1)} - V^{(r)}) . \sin \frac{1}{2} V^{(r+1)} + V^{(r)})$; and for the first division $u^{(1)} = b . \sin V^{(1)}$, and $z^{(1)} = 2b$

$2b \,.\, \sin^2 \frac{1}{2} V$(1). The depression of the mercury in the barometer is $\frac{1}{ab}$. (*Suppl. Th. de l'Act. Cap.* p. 59 . . .) The formulæ for z, and for $\frac{dz}{du}$ (p. 60) give the following series, which have been employed for depressions less than ·8 millimetre: $z = \frac{1}{ab}\,(\overline{2}\cdot 8860566 \,.\, u^2 + \overline{3}\cdot 1700532 \,.\, u^4 + \overline{5}\cdot 1018673 \,.\, u^6 + \overline{8}\cdot 7838040 \,.\, u^8 + \overline{10}\cdot 2719206 \,.\, u^{10} \ldots)$, and $\text{tang}\, V = \frac{1}{ab}\,(\overline{1}\cdot 1870866 . u + \overline{3}.7721132 . u^3 + \overline{5}.8800186 . u^5 + \overline{7}.6868940 . u^7 + \overline{9}\cdot 2719206 \,.\, u^9 + \ldots)$: the coefficients of the powers of u being here represented by their logarithms, for the assistance of the calculation; and the figures distinguished by a horizontal stroke being negative indices. In this manner, by assigning to u such a value that V should be between four and six decimal degrees, the corresponding ordinates have been very accurately determined for this inclination of the end of the curve: hence, by means of the preceding formulæ, these ordinates have been calculated for V = 6°, and this angle has been successively increased by two degrees at each step still it became 12°, and then by four degrees as far as V = 52°.

In order to arrange the results obtained by this method in a table calculated for equal increments of the diameter of the tube, I have observed that the differences of the logarithms of the depressions form in this case a series which varies but very slowly. By means of this property, I have obtained without difficulty the following table, the principal calculations having been obligingly furnished me by Mr. Bouvard: and the same property will also serve for the interpolation of the table.

We find in Nicholson's Journal for October 1809, a similar table, formed by the expansion of the expressions for z and sin V into a series, and founded on elements differing but little from those which we have employed. These two tables agree very nearly with each other, and with that which Lord Charles Cavendish had deduced from experiments. But the method which we have just explained appears to me to be more accurate [??], and more convenient for calculation [??]; it has besides the advantage [??] of showing the influence of the variation of the angle of contact of the surface of the mercury with the tube on the capillary action. It is well known that by the friction of the mercury against the sides of the tube, and perhaps also by a peculiar viscosity of the liquid, the angle is liable to considerable variations: it diminishes sensibly when the barometer rises, and increases when it descends, the surface of the drop becoming more convex in the former case, and less convex in the latter. In order to restore the surface to its natural state, it is usual to strike the tube gently and repeatedly; but it is difficult to remove the irregularity altogether. Fortunately when the tube is very large, the variations of the angle of contact have but little effect on the depression of the summit, though they sensibly alter the height of the convex part. The method above explained affords us the means of appreciating this alteration. For, the depression remaining the same, it enables us to ascertain the addition made to the corresponding ordinates of the section of the surface in the last division of its length: and then, the angle of contact being supposed constant, it gives us the variations of the extreme corresponding ordinates, with reference to a given variation in the depression. It is easy to infer from these determinations, by the methods of the differential calculus, the variations of the depression of the summit of the drop, and of its height, depending on a given variation of the angle of contact; and consequently the variation of the depression corresponding to an observed variation of the height of the drop. I find in this manner, that in a tube 11·4 millimetres in diameter, a diminution of one-tenth of a millimetre, in the height of the convex portion, produces in the depression depending on capillary action a diminution of ·015; that is, about one-seventh as much as the diminution of the height.

Table of the Depressions of the Mercury in the Barometer derived from its capillary Action, expressed in Millimetres.

Diam.	Depr.	Diam.	Depr.	Diam.	Depr.
2	4·5599	9	·5354	15	·1245
3	2·9023	10	·4201	16	·0970
4	2·0388	11	"·3506"	17	·0754
5	1·5055		[·3306]	18	·0586
6	1·1482	12	·2602	19	"·0430"
7	·8813	13	·2047		[·0450]
8	·6851	14	·1597	20	·0352

[*Table calculated and interpolated from the preceding for English inches, according to the method employed by the author, and compared with the Table published in* Nicholson's *Journal, and there considered as generally accurate to the last place of decimals: the ultimate product of the depression and the bore being assumed* ·015, *and the angular extent of* *the arc* 47° 20′, *while M.* LAPLACE'S *Elements are* ·01469 *and* 46° 48′.

Diam.	Depression.		
	NICH. 1809.	LAPL. 1810.	L^{D}. C. C. 1776.
·8	·00118	·00128	
·7	·00224	·00244	
·6	·00416	·00462	·005
·5	·00805	·00868	·007
·45	·01106	·01174	
·4	·01522	·01591	·015
·35	·02098	·02165	·025
·3	·02906	·02965	·036
·25	·04067	·04117	·050
·2	·05802	·05798	·067
·15	·08621	·08538	·092
·1	·14027	·01394	·140
·05	·29497		

A. B. C. D.]

XXIII. *On Meteorological Nomenclature.* *By* P. J. BROWN, *Esq.*

To Mr. Tilloch.

SIR,—WANT of leisure has prevented my perusing, till this evening, Mr. Forster's paper on the nomenclature of clouds published in your last number, and I regret being compelled to differ in opinion from a person for whose talents I have so much respect. In every department of polite literature, with the exception of scientific nomenclature, I feel decidedly hostile to the late fashionable introduction of exotic words*: the writings of Swift, whose language must be admired by every lover of pure English, have fully proved the adequacy of our mother tongue, for every purpose of general composition: but it should be recollected, in adopting scientific terms, that science is not a native of any particular country; but a citizen of the world who will always be protected and fostered in every state alive to its own interest and glory: it is consequently evident that the language of science should be, to as great an extent as can be accomplished, intelligible to all.

Consider the state of chemical knowledge when the illustrious Lavoisier composed his excellent nomenclature: can it be supposed that the acquisition of that most pleasing and useful science would have become so general, if, instead of adopting terms easy to be comprehended by all persons of moderate education, he had rummaged the old neglected Norman-French for a set of uncouth epithets which would have *posed an antiquary?* Suppose that the still more illustrious Linnæus, instead of his most admirable, comprehensive, and comprehen*sible* terms, had published his *Systema Naturæ*, his *Genera* and *Species Plantarum*, with a scientific nomenclature extracted from the barbarous radicals of the Swedish language, can it be for a moment thought that the study of nature, and consequent expansion of the human mind, would have been so universal as it happily has been?

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* Addison's excellent and humourous paper in the Spectator, No. 165, is as applicable to the present day, as it could possibly have been to the time of the battle of Blenheim: how many dispatches during the late war would have left half London ignorant of the fate of a battle, had not the guns told them in more intelligible terms that our brave countrymen had gained the victory!

In illustration, let us consider the languages of botany and mineralogy: a person possessing no more knowledge of the learned languages than is necessary for the proper comprehending of our own, without being able to translate a single line of Virgil or Horace, may (from the universal adoption of the Linnæan phraseology) in a very short time be able to take up the Flora of almost any country, and stroll with its author through paths of flowers; now investigating the scanty herbage of the northern regions, and then revelling in the magnificent productions of New Holland or the Cape. Mineralogy on the contrary, whose connexion with chemistry, whose durable productions abounding with beauty, brilliancy, and the most correct geometry, would, under more favourable circumstances, have been studied with delight, is comparatively neglected; and may not the neglect be in a great measure attributed to the jargon with which it is incumbered? a jargon unintelligible to any but the Germans, from whom it sprung, and the few who have degraded themselves by their conversion into Germanised Englishmen! Had the state of chemical and mineralogical knowledge been the same in the time of Linnæus as it is at present, he would most probably have occasioned the extensive cultivation of the latter, by bestowing on it a language brilliant and exact as its own native crystals; whereas the student is now enveloped in a fog, dark and impenetrable as the recesses from whence those crystals are derived.

If Mr. Forster's meteorological observations were intended for the information of his countrymen only, and were made known to them through a medium the circulation of which was confined to the limits of our own isle, there could be no objection to his conveying instruction in the terms he proposes; but when it is recollected that your valuable Magazine most deservedly attracts the attention of those citizens of the scientific commonwealth who reside abroad*; I should hope that, on reconsidering the circumstance, it will appear to him as unadvisable to force a set of Saxon terms upon our fellow-labourers on the continent, as it would be in ourselves to injure our masticating apparatus, by ineffectually endeavouring to become masters of the German mineralogical dissonance †.

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* Mr. F. admits the propriety of Mr. Howard's nomenclature in descriptions in Latin, or which are to go abroad: the latter is actually the case with

I by no means wish this to be considered as the commencement of a controversy, but as containing a statement of the opinion of one whose want of knowledge is, perhaps, only equalled by his regret that his knowledge is not more extended.

I have the honour to be, sir,
Your most obedient humble servant,

Old Brompton, Feb. 12, 1816. **P. J. Brown.**

P. S.—The snapping of sulphur when held in the warm hand, as noticed by your correspondent J. F., is a property which has long been known: it is mentioned by several authors to whose works I have not the opportunity of immediate reference; but I can venture from memory to refer to the lesson on Sulphur in Lagrange's Manual of Chemistry: for J. F's ingenious observations on the subject he has full credit.

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with those inserted in your Magazine: and with respect to the difficulty of impressing on the minds of mere English readers, names derived from the Latin, it is presumed that the term *cumulus* will as readily convey the idea of a heaping up, or accumulation (*ad cumulus*) of clouds, as the proposed one of *stackencloud*: indeed, if new terms must be acquired, the trifling additional difficulty (*should any exist*) will be no impediment to those stimulated by a *desire* to learn.

† As many of your young readers who are pleased with the study of mineralogy may not have entered into its investigations five years ago, I shall consider no apology necessary for recommending to their perusal Mr. Chenevix's "Reflections on some Mineralogical Systems," published in your xxxvith and following volumes. In volume xxxvith, and last paragraph of page 379, I would suggest a manuscript reference to be made to vol. xlii. page 25, where Professor Stromeyer's Observations on the Composition of Arragonite will add force to Mr. C.'s arguments; by removing the only objection of any weight which remained opposed to the system of M. Haüy. Newton incurred the ridicule of the pseudo-philosophers of his day, by arguing *from theory*, that diamond ought to be inflammable, and that water itself should also contain much inflammable matter. Those whose ridicule he experienced had long rested in their graves, when the brilliant discoveries of modern chemistry proved the correctness of his assertion. Can M. Haüy's principles need a more honourable comparison? They pointed out to him that arragonite *ought* to differ from carbonate of lime; the most celebrated chemists of the early part of the 19th century were long unable to detect the difference; it is at length discovered, to the immortal honour of the French philosopher, and I trust to the sad discomfiture of the partisans of *external characters and mineralogical instinct*.

XXIV. On

XXIV. *On the Cosmogony of Moses; in Answer to the Strictures of* F. E——s[*]. *By* J. C. Prichard, *Esq.*

To Mr. Tilloch.

Sir, — I feel it incumbent upon me to reply to some strictures contributed by a correspondent to the last number of your Magazine, on a paper of mine which you did me the honour of inserting in the Philosophical Magazine for October, and which contained an attempt to illustrate the wonderful and striking conformity that exists between the series of events recorded in the beginning of Genesis, and the result of geological researches into the crust of our globe.

The question turns upon the sense we ought to affix to the words interpreted the "days" of the creation. I have endeavoured to show that, if we receive the expression as designating indefinite periods of time, the whole account bears the most important relations to the epochs of nature. I will add that, if we pay no regard to the genius of Hebrew literature, but are determined to interpret the writings of Moses according to the strictest rules which the idioms of our western languages impose, we shall distort the sense of many sublime passages; and in this particular instance, in the place of a true detail of events, which we know to have really happened, shall substitute a relation improbable in itself, and wholly irreconcilable with the certain results of sensible evidence.

But to proceed to the argument. Your correspondent seems willing to allow that the word "day" might properly bear the sense I have affixed to it, did it not occur in conjunction with the terms which designate the natural commencement and termination of a day, viz. morning and evening. Now, this circumstance appears to me to occasion no difficulty whatever. Nothing is more common than this mode of expression, even in languages of so stiff and unbending a texture as the modern European dialects. If we use the word *day* to signify a portion of time, and have occasion to allude to the beginning or end of the period designated, we always carry on the metaphor, and adopt the corresponding terms. We continually hear such phrases in rhetorical or poetical language as "the *evening* of our *days*," the "*morning*," "*noontide*," and "*evening*" of human life. The poet Gray uses the very terms in dispute, when alluding in one of his poems to the beginning and termination of Richard the Second's reign. He says "Fair laughs the *morn*," &c.—

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[*] See our last number, p. 9.

"the gilded vessel goes,"—"regardless of the sweeping whirlwind's sway,"—"that hush'd in grim repose expects his *evening* prey." It will be said that this is the peculiar style of poetry: but is it possible to doubt that the former part of Genesis is a poetical composition? Have not the first chapters all the manner and decoration of the sacred poetry of the East,—not indeed of the poetry of fiction, but of the poetry of elevated feeling, and of that style which sets forth great moral truths by means of striking images in a manner fitted to produce their full impression? However, a parallel passage may easily be found in plain English prose. The following is from Sir Walter Raleigh: "The Devil is now more laborious than ever, the long *day* of mankind drawing towards an *evening*." I doubt not that I could find a hundred such examples if I had leisure to look for them.

Perhaps it may be thought more important in the present case to remark, that in the most ancient books of the Oriental nations which remain to our times, excepting only the writings of Moses, the metaphorical use of the word *day* is very common; and, what is more to our purpose, the words which designate the natural terminations and divisions of a day, as night or evening, and twilight or morning, are accurately specified, the metaphorical sense being still carried on. But before I proceed to adduce examples of this kind, I must obviate an objection that will immediately be made to any reference to such sources in the present question. It will be asked, What relation can the books of other Eastern nations have to the Hebrew Scriptures, which are the productions of inspired writers? To this I answer, that the Oriental nations have exhibited from very remote times a general resemblance, a common character in their modes of thinking and acting; that the same conformity may be expected, and really exists, in their manner of expressing their thoughts, and in the style of their compositions; and therefore that the writings preserved by one of these nations from distant ages may afford illustration with respect to the use of language among the others. But further, I am persuaded that some of the compositions I refer to, have a much nearer connexion with the early writings of the Hebrews than is universally apprehended.

Many biblical scholars have held the opinion that the early part of Genesis consists of records preserved from periods of very remote antiquity, and adopted by Moses as furnishing an authentic sketch of the principal events which had happened in the world down to the time of Abraham, when the particular history of the Hebrews begins, and immediately assumes the character of a contemporary and circumstantial narrative. I confess that this opinion seems to me to be supported by several strong arguments: but in maintaining it I do not place the author of the Pentateuch in the rank of common compilers of historical fragments possessed merely of natural intelligence; nor do I apprehend that the authority of the records themselves can be diminished by assigning them to an age as near as possible to the events recorded: neither do I regard them in their origin as common historical testimonies. The cosmogony cannot be a piece of common history resting on human testimony. It must certainly be either a fragment of the imagination, or a production which owed its origin to some supernatural intelligence; and that it is not a mere figment, I am convinced by the accuracy with which it details the succession of the epochs of nature.

The only argument which I shall notice in proof of the opinion above mentioned, is the great diversity of style which has been traced in various portions of the ante-Hebraic history, and the solution which this hypothesis affords of a phænomenon which is wholly inexplicable on any other; viz. the remarkable connexion discovered between the primitive histories of the most remote nations on the earth, and these documents embodied in the Genesis. The facts I allude to are to be found in many authors, and I need not detail them here; though, as it will presently appear, they bear a near relation to the subject of this paper. It is in vain to attempt to account for this coincidence, in the manner which some of the Fathers and Hyde and Prideaux have pursued, as by converting Abraham into Brahma; or by making Zoroaster a renegado Jew; since not only the Asiatic nations, but the Runic scalds of Iceland and Scandinavia, and the ancient priests of Mexico, were equally in possession of the primitive traditions; and the latter certainly never obtained them from Jerusalem.

These phænomena can only be solved by going back to the first periods of human society. There are many circumstances which indicate that certain records were preserved from antediluvian times. That alphabetic characters were known before the deluge, is not very probable; but that hieroglyphic or perhaps picture writing was practised, does not seem inadmissible, when we consider that these arts are found among nations in a very rude state of society, as the Canadians and South American Indians. It may reasonably be supposed that the true interpretation of such memorials was preserved among some nations, and lost or diversified among others; nor is it to be doubted that the Hebrews retained the genuine sense, as they also preserved

served in purity the primitive religion. We may thus account for the different representations which we often find of the same historical facts or moral truths.

We have, moreover, direct historical testimony of the existence of antediluvian records. Many ancient authors, particularly St. Jude and Josephus, allude to writings which were attributed to the patriarch Enoch: and though I am not disposed to advocate the authenticity of any such productions, yet the frequent mention of them proves that the Hebrews were persuaded that some writings had survived the flood. But Berosus, whose Chaldean history of the ten antediluvian generations differs but little from that of Moses, expressly affirms that Xisuthrus or Noah, after the intimation given him of the catastrophe that was approaching, diligently compiled records of the former history of the world, from which all existing accounts are said to have been derived.

We are thus enabled to account for the fact that scarcely any thing is contained in the antediluvian history of Moses, which may not also be found, though more or less embellished, in the records of other nations, particularly in those of the Hindoos. Thus in the Institutes of Menu, as translated by Sir W. Jones, there are some passages relating to the epochs of the creation; and it is chiefly for the sake of quoting these that I have made so long a digression.

That Menu or Satyavrœta was Noah, is certain from the account of his escape from an universal deluge, which agrees in general with that of Moses, containing also some of the fabulous circumstances mentioned by Berosus. For the Hindoo story I refer your readers to the extracts given in several parts of Sir W. Jones's works; and need only quote the following words, in which Vishnu is said to have foretold the approaching catastrophe. "In seven days all creatures which have offended me shall be destroyed by a deluge; but thou shalt be secured in a capacious vessel miraculously formed: take, therefore, all kinds of medicinal herbs and esculent grain for food; and together with the seven holy men, your wives, and pairs of all animals, enter the ark without fear*," &c.

The Institutes of Menu begin with an account of the creation, which bears a strong resemblance to that of Moses, though embellished or deformed by many wild conceits. The spirit of God moving on the face of the waters is expressed in the same terms. Menu afterwards proceeds to define the periods which are termed days and nights. He says, "Learn now the duration of a day and night of Brahma and of the several ages," &c.—"Sages

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* Translated from the first Purana.

have given the name of Crita to an age containing 4000 years of the gods; the *twilight* preceding it consists of as many hundreds, and the *twilight* following it of the same number," &c.—"And by reckoning a thousand such divine ages, a day of Brahma may be known: his *night* has also an equal duration." —"At the close of his *night*, having long reposed, he awakes, and awaking exerts intellect."—"Intellect, called into action by his will to create worlds, performs again the work of creation." (*Institut. Menu*, by Sir W. Jones, chap. 1.)

The relation of this account to the Mosaic cosmogony, and to the particular object for which I have been induced to cite it, appears to me so obvious, that I cannot help thinking it will be considered by every unprejudiced person as affording a strong confirmation to the interpretation I have endeavoured to maintain, especially if it be allowed that Moses founded his narrative on an antediluvian record, and that another similar relic furnished the basis of the Hindoo legend; for it cannot be imagined that the story, as it now stands, was the work of Menu or Noah. The application of the words *twilight* and *night* is to be particularly remarked.

I shall not attempt to trace the record of the cosmogony among the different nations who preserved vestiges of it, but shall content myself with adducing one more example: it is found in the scanty relics of the literature of the Etruscans, a people whose early history is lost in the dark night of antiquity. Suidas* informs us that there was a very ancient Etruscan historian of great authority, in whose works was found an account of the creation of the world, which he said was divided by the Creator into six departments, each occupying the space of a thousand years. In the course of the first chiliad or millennium he created the heaven and earth; in the second, the visible firmament; in the third, the waters of the ocean and those contained in the earth; in the fourth, the great luminaries of heaven; in the fifth, vegetables, and all kinds of animals; and in the sixth and last, man. The same remark which I made above on the Hindoo legend may be applied still more forcibly to this account preserved by the Etruscans.

From these considerations I think we may conclude it to be highly probable, that the words of the first chapter of Genesis are to be interpreted in a tropical sense. But, even if it should be made to appear that the words and phrases will not bear this construction, according to the natural forms of language, still I should by no means concede that we are to understand the cosmogony according to the vulgar acceptation; and I should support this opinion on the following grounds.

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* Suidas in voce Τυῤῥηνια.

There are many parts of the ancient Hebrew Scriptures which it is obviously necessary to understand, and which are to be understood according to the judgement of the most learned and orthodox critics, in a metaphorical manner; not by mere changes in the force of particular words, but by figurative applications of the sense of whole passages, the real meaning being contained in some allusion, and expressed by a type, which under a sensible and striking image conveys a more abstract truth. It is well known that a similar method of illustration prevailed extensively among the philosophers of the East, from whom the Greeks learnt to divide their doctrines into esoteric and exoteric, a practice which was introduced by Pythagoras and Plato, and followed by Aristotle and others. We are well assured that the learned among the Jews viewed many parts of their ancient Scriptures in this light; and, after the captivity of their nation at Babylon, in which calamity a considerable portion of their literature was lost, and the explanations of many things apparently forgotten, which had been preserved among the priests, set themselves to make up the deficiency. The extent of the Talmud, and the great authority which this compilation acquired, notwithstanding its numerous absurdities, proves how firmly they were persuaded of the esoteric sense contained in many parts of the biblical writings. At the Christian æra there was only one sect, viz. the Sadducees, who wholly followed the literal meaning, and they were held in no great estimation: and we learn from St. Paul, that the Pharisees, who allowed the figurative sense, had the truest interpretation of the Jewish discipline. It was in this path that Hillel acquired so much fame, as well as his descendant and follower Gamaliel. Philo[*] the learned Jew, and Josephus, assure us that many passages must be explained in this tropical or figurative way; and, what is very remarkable and strongly in favour of my argument, they both make an express observation to this effect, with reference to the six days' work of the creation. It cannot be denied that this mode of interpreting has led to many abuses, and that Origen and others, by explaining away the historical facts of the Old Testament into types and metaphors, converted history into a mass of mystical absurdity. But this is no argument against the limited and judicious application of a method which is sanctioned, as we have seen, by the highest authorities among the Jews themselves and which has been acknowledged by all Christian writers to be necessary for the explanation of many passages. Thus, for example, what sense, except the most trifling one, can be made of the following passage, if we are obliged to adhere to the obvious meaning of the words? "I will put enmity between thee and the woman, and between her seed and thy seed; it shall bruise thy head, and thou shalt bruise his heel."

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[*] *Philo Judæus in Cosmopœia item in Allegoriis.*

It being certain that we are to look for an esoteric sense in some passages, there is none which more evidently requires such an interpretation than the cosmogony. Had Moses read a lecture on geology to the shepherds of Goshen, and told them what space of time each oceanic deposit occupied, and by what organic remains it is to be recognised, he would have spent his time to little purpose. His object, doubtless, was to declare that the universe was the work of the Almighty Creator, and to set this truth in a point of view the most striking and impressive. At the same time, by mentioning a series of events which really took place, in the exact order in which we are now assured that they actually happened, he has left us a proof that he was not uttering a rhapsody the mere offspring of his imagination.

But if we are determined to adhere to the vulgar acceptation, we shall, as I said before, establish a total and irreconcilable schism between the Mosaic relation and the evidence of indubitable facts.

No man who has witnessed the proofs to which geology refers, and who is capable of putting two propositions together, can refuse to admit this conclusion. In traversing a mountainous district no idea forces itself more irresistibly on the mind, than the vast periods of time which must have elapsed during the gradual deposition and consolidation of the immense series of beds which constitute all the highest regions of the globe. These phænomena attest indisputably an æra of incalculable extent before the first efforts of living nature. The second epoch, viz. that of zoophytes and vegetables, must have comprehended a vast concourse of ages. Pallas mentions a succession of limestone beds containing encrinites and other zoophytes, which lie one upon another to the extent of 60 miles. The whole of Wales belongs to this æra; for I have seen impressions of zoophytes and testacea on the summit of Snowdon, which is the oldest part of the district, and the newest part is occupied by the coal-formation. What a lapse of time must the successive deposition of all the rocky beds which form this district have occupied! Again, if we proceed from the centre of South Britain, in a direct line towards Paris, we tread at every step from older to newer beds. We begin with the oldest beds, which belong to the third epoch, and contain the remains of fishes; and we reach the Paris basin before we discover the oldest vestiges of quadrupeds; and there we only find the bones of *Palæotheria* and other *Pachydermes*, which seem to have lived their day, and to have become extinct long before the surface of the earth became fitted for the reception of man, or even of the quadrupeds which are subservient to his use, and which would seem to have immediately preceded his appearance, since we find their relics only in alluvial grounds.

With respect to the successive extinction of these races of animals, it is a fact easily explained. As the water of the ocean became prepared for more perfect creatures, it may be supposed that it was unfit for sustaining those to which its former qualities had given birth. I have elsewhere endeavoured to show that all land animals had originally a local and determinate seat on the globe. Confined by natural barriers within the limits of their native regions, many of them seem to have awaited the hour of their destruction by means of inundations and other catastrophes.

On the hypothesis proposed by your correspondent, I shall only remark that, although purely conjectural, it is liable to an objection drawn from physical considerations. A number of curious coincidences render it highly probable that the orbital motions of the planets and their motions of rotation were produced by the same physical cause, and were therefore simultaneous in their commencement. The circumstances I allude to are set forth in a most luminous and striking manner by Laplace in his *Systême de la Nature.* But it is futile to enter into a discussion of what *may have happened* before the creation of the sun.

Having thus fully stated the grounds of the opinion I formerly ventured to offer, I shall decline all further controversy on this subject.

I have the honour to be, sir,
Your obedient servant,
Bristol, Feb. 12, 1816. J. C. PRICHARD.

XXV. *On Safe-Lamps for Mines.*

To Mr. Tilloch.

SIR, — IN the month of November last I had the pleasure to communicate to you the result of several successful experiments, made in the presence of the Literary and Philosophical Society here, with the safe-lamp invented by Mr. Stephenson, which, I am happy to add, has been since used in the most dangerous parts of some coal-mines without any accident having occurred. On Tuesday the 6th instant Sir H. Davy's recently improved lamp, the flame of which is encompassed by wire-gauze, was also exhibited by a professional gentleman who had previously tried it in Walls End and Hebburn collieries; and its merits appear to be still greater than those of Mr. Stephenson's. The lamp being suspended in a vessel of glass open at the top, and the carburetted hydrogen admitted from below, the bright flame of the wick nearly disappeared, but the cylinder of wire-gauze was filled with a feeble but steady greenish light. On a greater volume of inflammable air being thrown in, the flame gradually died out. Results more satisfactory could not be expected nor wished for, particularly when we were assured that these accorded with numerous trials made in the most hazardous drifts of our coal-mines.

Notwithstanding all that has been lately said in some of the periodical publications, respecting the obstinacy of the viewers employed here, and the stupidity of their under agents and pitmen, you may depend upon it that these safe-lamps are hailed by this class of people as a most fortunate discovery, which will soon be adopted by them in every mine infected with fire-damp. And could a mode be struck out, of preventing inflammation taking place by means of the furnace placed at the bottom of the upcast shaft to accelerate the circulation of air through the workings, little would be wanting to render the occupation of the collier as safe at least as that of the persons employed in lead and copper mines.

Your most obedient servant,

Newcastle-upon-Tyne, Feb. 16, 1816. N.

XXVI. *On the Tides.* *By M.* LAPLACE*.

[*Read to the first Class of the Institute the* 10*th of July* 1815.†]

THIS phænomenon particularly merits the attention of observers, both because it is the nearest and most perceptible effect of the action of the heavenly bodies, and because the numerous varieties it presents are well calculated to verify the law of universal gravitation. At the request of the Academy of Sciences a course of observations were made at the beginning of the last century in the port of Brest, which were continued during six successive years, and of which the greater part have been published by Lalande in the fourth volume of his *Astronomie*. The situation of the port is very favourable for observations of this kind. It communicates with the sea by means of a canal, terminating in a very large road, at the extremity of which the port has been built. Thus the irregularities of the motion of the sea are considerably weakened before they reach the port, very nearly in the same manner as the oscillations produced in the barometer by the irregular motion of a vessel, are diminished by a contraction in the tube of this instrument. In other respects the tides being considerable at Brest, the casual variations occasioned by winds form only a small part of them. It may also be remarked in the observations made of these tides, however few there may be of them, that a great regularity prevails which is not altered by the little river, which loses itself in the immense road of this port. Struck with this regularity, I solicited government to order a new course of observations to be made at Brest, during an entire period of the motion of the nodes of the lunar orbit. They had long been wished for. These new observations are dated from the 1st of June 1806, and since that period they have been continued uninterruptedly to this day. There is still, however, much wanting. They relate neither to the same part of the port, nor to the same scale. The observations of the first five years have been made at the place called *La Mâture*, the others were taken near the bason. I observe that this change has produced only slight differences; but it would have been better, undoubtedly, if all the observations had been made at the same place and upon the same scale. It is time, indeed, that phænomena of this nature should be observed with the same care as those of astronomy.

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* From the *Connoissance des Tems* for 1818.

† For this translation we are indebted to T. S. Evans, jun. of the College school, Gloucester.

In these new observations I have considered those of the year 1807 and of the seven subsequent years. In each equinox and in each solstice I have chosen the three syzygies and the three quadratures nearest to this equinox and this solstice. In the syzygies I have taken the excess of the high water of the evening above the low water of the morning of the day which precedes the syzygy, of the day of the syzygy, and of the four following days, because the highest tide happens about the middle of this interval. I have made a sum of these excesses corresponding to each day, by doubling the excesses which relate to the intermediate syzygy, or that nearest to the equinox or the solstice. By this means the effects produced by the variation of the distances of the sun and of the moon from the earth are destroyed: for if the moon were, for example, towards its perigeum in the intermediate syzygy, it would be near its apogeum in the two extreme syzygies. The sum of the excesses thus obtained are, therefore, very nearly independent of the variations of the motion and of the distances of the heavenly bodies. There are still inequalities of the tides, different from that inequality, whose period is about half a day, and which in our ports is much greater than the others. For by considering at the same time the observations at the two equinoxes and at the two solstices, the effect of the small inequality, whose period is nearly a day, is mutually destroyed. The sums in question are consequently entirely owing to the great inequality. The winds can have little influence on them; for, if they raise the high water, they must equally depress the low water. I have determined the law of these sums for each year, by observing, that their variation is very nearly proportional to the square of their distance in time from the maximum which has given me this maximum; its distance at the mean of the times of the syzygy tides, and the coefficient of the square of the times in the law of the variation. With regard to this coefficient, the little difference which the observations of each year present, proves the regularity of these observations: and according to the laws which I have elsewhere established, on the probability of results deduced from a great number of observations, some judgement may be formed of the accuracy of results determined from the whole of the observations of eight years.

In the same manner I have considered the quadrature tides, by taking the excess of the high water in the morning above the low water of the evening of the day of the quadrature, and of the three following days. The increase of the tides, beginning from the minimum, being much more rapid than their decrease, beginning from the maximum, I have thought it necessary to confine the law of the variation proportional to the square of the time within a much shorter interval.

In all these results the influence which the declinations of the heavenly bodies have on the tides, and upon the law of their variation in the syzygies and in the quadratures, is evidently shown. In considering, by the same method, eighteen equinoctial syzygy tides towards both the perigeum and the apogeum of the moon, the influence which the changes of the lunar distance have upon the height and upon the law of variation of the tides, is manifested with the same degree of evidence. It is thus that by combining observations in such a way as to bring out every element, which we are desirous of knowing, we are able to separate the laws of the phænomena when mixed and confounded together in the collections of observations.

After having obtained the results I have just mentioned, I compared them with the theory of the tides delivered in the fourth book of the *Mécanique Céleste*. This theory is founded on a principle of dynamics, which renders it very simple, and independent of the local circumstances of the port, which circumstances are too complicated for the possibility of submitting them to calculation. By means of this principle, they enter into the results of the analysis as arbitrary quantities, which ought thus to represent the observations, if the universal gravitation is in fact the true cause of the tides. The principle is this: *The state of a system of bodies in which the primitive conditions of motion have disappeared by the resistance it meets with, is periodical, as well as the forces which animate it.* By reuniting this principle to that of the coexistence of very small oscillations, I have obtained an expression for the height of the tides, of which the arbitrary quantities comprise the effect of the local circumstances of the port. To deduce this, I have reduced the generating expression of lunar and solar forces acting upon the ocean into a series containing the sines and cosines of angles increasing proportionally to the time. Each term of the series may be considered as representing the action of another object, which moves uniformly at a constant distance, in the plane of the equator. Thence arise several kinds of partial tides, the periods of which are about half a lunar day, a day, a month, a half-year, a year; and lastly, eighteen years and a half, which is the duration of the periodical motion of the nodes of the lunar orbit.

In the book which I have quoted of the *Mécanique Céleste*, I have compared this theory with the observations made at Brest at the commencement of the last century; and I have determined the constant arbitrary quantities relative to this port. I was curious to see whether these circumstances were found to be the same by the observations made a century afterwards, or whether they have experienced any alteration by the changes which the operations of nature and art have produced, either at the bottom of the sea or in the port, and on the adjacent coasts. The result of this inquiry is, that the actual heights of the tides, in the port of Brest, surpass the heights determined by the old observations by about $\frac{1}{4}$th. One part of this difference may arise from the distance of the points where these observations were made; another part may be attributed to the errors of the observations: but these two causes do not seem to me sufficient to produce the whole difference which indicate with great probability a secular change in the action of the sun and moon on the tides of Brest, if we could be well assured of the exactness of the graduations of the old scale, and taking into account its inclination to the horizon. But the uncertainty we are in with respect to this point, does not permit us to pronounce upon this change, which ought in future to fix the attention of observers. In other respects the agreement is surprising between the old and the modern observations, as well as the theory; with regard to the variations of the heights of the tides depending on the declinations and distances of the heavenly bodies from the earth, and the laws of their increase and decrease in proportion as they recede from their maximum and from their minimum. In the *Méçanique Céleste* I had not considered those laws relatively to the variations of the distances of the moon from the earth. Here I take them into consideration, and I find the same agreement between the observation and the theory.

The retardation of the greatest and least tides which follow the times of syzygies and quadratures, was observed by the ancients themselves, as we read in Pliny the naturalist. Daniel Bernouilli, in his paper on the Tides, that gained the prize proposed in 1740 by the Academy of Sciences, attributes this retardation to the inertia of the water; and perhaps also, adds he, to the time taken by the action of the moon to transmit itself to the earth. But I have proved in the fourth book of the *Mécanique Céleste*, that by allowing for the inertia of the water, the highest tides would coincide with the syzygies, if the sea covered uniformly the whole surface of the earth. As to the time of the transmission of the action of the moon, I have discovered by a comparative view of the whole of the celestial phænomena, that the attraction of matter is transmitted with a velocity incomparably greater than even the velocity of light itself. We must therefore seek some other cause for the retardation in question. I have proved in the book quoted above, that this cause is the rapidity of the motion of the celestial body in its orbit, combined with the local circumstances of the port. I have remarked, moreover, that the same cause may increase the ratio of the action of the moon on the sea to that of the sun; and I have given a method of determining this increase by means of the observations, the idea of which is this: Let us suppose the motion of the sun to be uniform:—if we consider only the great inequality of the tides whose period is about half a day, the solar tide is decomposed very nearly into two others, which are exactly those that would be produced by two celestial bodies moving uniformly, but with different velocities, in the plane of the equator, at the mean distance of the sun from the earth. The mass of the first body is that of the sun, multiplied by the cosine of the inclination of the ecliptic to the equator: its motion is that of the sun in its orbit. The second body constantly corresponds with the spring equinox, and its mass is that of the sun multiplied by the half of the square of the sine of the obliquity of the ecliptic. At the equinox these bodies are either in conjunction or in opposition, and the tide is the sum of the tides produced by each of them:—at the solstice the bodies are in quadrature, and the tide is the difference of these partial tides. The observations of the solar tide in these two points show, therefore, the relation of the partial tides, and consequently the ratio of the actions of the heavenly bodies on the ocean; and by comparing it to the ratio of their masses, the increase produced on it by the difference of their motion will be determined.

This increase is almost insensible for the sun, on account of the slowness of its motion; but it is very evident for the moon, whose motion is thirteen times more rapid, and whose action on the sea is nearly three times greater.

By comparing in the fourth book of the *Mécanique Céleste*, the observations of equinoctial and solstitial tides in the syzygies and the quadratures, I was led by this method to an increase of at least a tenth in the ratio of the action of the moon to that of the sun; but I remarked that an element so delicate ought to be determined by a greater number of observations. The collection of modern observations has procured me this advantage. These observations, employed in double number, confirm the increase indicated by the ancient observations, and they make it more than one-eighth. Another method founded on the comparison of the tides towards the apogeum and perigeum of the moon, and applied to the ancient as well as modern observations, leads us also to a similar result.—Thus the increase of the action of the heavenly bodies on the tides in the port of Brest ought not to leave any doubt.

The results of observations being always susceptible of errors, it is necessary to know the probability that those errors are contained within given limits. It is conceived, and with truth, that the probability remaining the same, those limits are the more diminished as the observations are more numerous, and agree better with each other. But this general view of the subject is not sufficient to warrant the exactness of the results of observations and the existence of regular causes which they seem to point out. Sometimes, indeed, it has induced us to seek for the cause of phænomena which were only the accidents of chance. The calculation of probabilities can alone enable us to appreciate these objects, which renders its use of the highest importance in physical and moral sciences. The preceding researches afforded me an opportunity too favourable to be neglected, of applying the new formulæ which I have obtained in my *Théorie analytique des Probabilités*, to one of the grandest phænomena of nature. I there explain at full length the application I have made of it to the laws of the tides. My object has been, not only to confirm the truth of those laws, but to trace the way which must be pursued in applications of this kind. Among these laws, the most delicate are those of the increase and decrease of the tides towards their maximum and their minimum, and the influence which the declinations of the heavenly bodies and the variation of their distances from the earth, exercise in this respect. It is evident that these laws are determined by the observations with extreme precision and probability, which explains the remarkable agreement between the results of modern observations with those which the old observations had given me, and with the theory of gravitation. According to this theory, the action of the moon on the sea follows the inverse ratio of the cube of its distance from the centre of the earth; and this law represents the observations of the tides with such exactness, that by these observations alone the law of attraction being reciprocally as the square of the distances might be determined.

My principal desire was to apply my formulæ of probability to the increase of the action of the moon on the tides, depending on local circumstances. To determine this, the preceding observations have furnished me with sixteen equations of condition, and from them I have determined this increase to be equal to the $\frac{1353}{10000}$ part of the action of the moon on the ocean. By applying my formulæ to this result, I find that there are 21,100 chances to one, that the local circumstances of the port of Brest increase the ratio of the action of the moon on the tides to that of the sun: this increase may therefore be considered as certain; but there are only fourteen chances to one that the preceding value is not in error one half. We must therefore wait for new observations to obtain it with great probability of being mistaken by only very small quantities.

The ratio of the actions of the moon and of the sun on the sea, corrected for the effect of local circumstances, is very important to be known, because it determines the coefficients; 1st, of the terrestrial nutation; 2dly, of the inequality of the precession of the equinoxes; and 3dly, of the lunar equation of the sun's motion. Newton and Daniel Bernouilli had deduced this ratio from the phænomena of the tides, but without having regard to the correction I have just spoken of, which they did not suspect. The ratio which I have determined, and corrected by the whole of the preceding observations, gives the mass of the moon equal to $\frac{1}{68 \cdot 7}$; that of the earth being unity. It therefore gives in sexagesimal seconds, 9·65″ for the coefficient of the nutation; which surpasses the coefficient determined by the observations of Maskelyne only by $\frac{5}{100}$ of a second. My formulæ of probability show that there are 21,400 chances to one, that the nutation is not below 9·31″, and there are the same number of chances to one, that it is not above 9·94″. According to this same ratio the coefficient of the inequality of the precession is 18·04, and that of the lunar equation of the solar tables is 7·56″, which differs from the coefficient that M. Delambre found directly from the examination of a great number of solar observations only by $\frac{6}{100}$ of a second. In this calculation, I have supposed the mean parallax of the sun equal to 8·59″, corresponding with that which I have deduced from my theory of the moon, compared with the inequality of the lunar motion, known by the name of *parallactic inequality*, and which M. Burckhardt has determined by means of a very great number of observations. M. Ferère, a learned Spanish astronomer, has lately confirmed this parallax by a new investigation of the transit of Venus in 1769; in which, by his own observations, he has corrected the latitude and longitude of places where this transit has been observed in America. The agreement between all these computations determined by phænomena so vague, is an additional confirmation of the principle of universal gravitation.

XXVII. *A new Instrument for comparing Linear Measures.*

By M. DE PRONY*.

THE comparison of linear measures, when great accuracy is necessary, requires careful and delicate operations, as well as the use of machinery not generally employed in commercial concerns, and difficult to be procured. I have already published the description of an instrument of this kind, invented and made by M. Lenoir, member of the Board of Longitude, which is as perfect as can be wished; but its expense and size put it out of the power of common observers to procure, and render it useless to travellers, who wish to know the proportion between some given standard, and the linear measures of any country through which they may pass.

I have had made for my own use, a *comparer*, which joins the two advantages of œconomy and portability: all the pieces of which it is composed may be fitted into a box of the size of a quarto book. The dearest part is a microscope; but even this requires nothing different from those with which observers are commonly furnished. In general it is only necessary to be at the expense of making three additional pieces, which I shall describe presently.

The properties and use of my *comparer* are founded on the progress which the art of dividing a right line has made within the last half century. This instrument has therefore, independently of the above advantages, that of requiring no *vernier* nor *micrometer screw*, &c.

It is well known that M. Richer, one of the first artists of Paris for the construction of mathematical instruments, has long been in the habit of making divisions on glass that are very clear and minute at the distances of 100ths of a millimeter, and even less *. A glass having two or three millimeters with the division of one of them into 100 parts costs at his house, ten or twelve francs.

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* Communicated by Dr. Evans.

Some foreign artists have also succeeded in this kind of work. I procured in my travels in Italy, two small discs of glass on each of which are two millimeters, the one divided into ten and the other into a hundred equal parts. I had them of M. Capello of Turin, an artist who is equally celebrated for his inventive mind and his ability to execute what he has conceived.

I know also at Paris an amateur, M. Le Baillif, who applies all the leisure moments which his situation under government affords, to the cultivation of the sciences; and who among his other talents possesses that of dividing a right line on glass in a rare and remarkable manner. He has had the kindness to trace for me on a small disc of glass 21 tenths of a millimeter; ten of which are subdivided into 100dths and 200dths. These divisions of 200dths are very neat, and perfectly visible with a microscope magnifying 100 times.

Those persons who wish to possess a *comparer* like mine, ought first to procure one of these glasses on which a right line is divided into as many millimeters as they please, and one of these millimeters subdivided into 100dths. The first 10, 20, 30, &c. strokes of this subdivision into 100, are prolonged, and the 5, 15, 25th, &c. should be also prolonged, but less than the former, in order that the tenths and half-tenths of a millimeter may be distinguished at the first glance of the eye.

The piece of which I have just spoken must be fixed at one of the extremities of a brass rule, the lengths of the strokes of the divisions being perpendicular to the length of the rule which carries at its extremity a steel stud, intended to be put in contact with the ends of the linear measures which we may have to compare.

Another fixed stud must be screwed and held very solidly on a board or a table which holds the measures and all the apparatus.

When we wish to compare any two linear measures, one of them must first be placed in such a way that one of its extremities may rest against the fixed stud, and its other end in contact with the moveable one; the whole being so disposed, that the axes of the rules, the axis of the linear measures, and the middle points of the divisions on the glass are exactly in right lines. The microscope held by the same board, or the same table on which the other parts of the apparatus and the linear measures are laid, must be so pointed to one of the divisions on the glass, that after some preliminary trials, or first approximative data, on the ratios of the linear measures, we may be certain that the second measure to be compared, when we have made the same dispositions respecting it, will bring the 100dths or 200dths of a millimeter under a *stroke*, which is afterwards to be determined when placed in the focus of the microscope; which microscope, when the collimation has been established, with respect to the beginning of the divisions, must be kept immoveable during the comparison of the two linear measures.

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* I have a micrometer screw made by this able artist, two decimeters in length. He engaged to cut these divisions at intervals of a half millimeter, and he succeeded so well that the most rigid proofs could not discover the least inequality that was sensible throughout the whole 200 divisions. This is one of the most difficult tasks that can be undertaken of this kind.

The apparatus is to be disposed in such a way, that the glass carrying the divisions may be placed between the reflecting mirror of the microscope and the object lens; and if we wish to adjust the focus-line to the line at the beginning of the divisions, we make the supports of the microscope to abut against the point of a fixed horizontal screw and nut; this support being made to slide along a rule parallel to the linear measures. The parallelism between the focus-line and the strokes of the division is easily obtained by the hand, by causing the microscope to turn round in the circular horizontal ring in which it is inclosed.

The collimation of the focus-line with one of the strokes on the divisions on the glass being thus well established, we remove the first linear measure, and replace it with the second, by resting the moveable stud against one of its ends, and making its other end to abut against the fixed stud. The point of collimation will change, if the measures are not equal; and their difference of length will be given by the quantity that the second measure shall have removed the first stroke of the divisions from the division on which the collimation was established for the first; which quantity of removal is measured by the number of millimeters, and 100dths of a millimeter, contained between the two successive points of collimation.

I return to the line placed in the focus of the microscope. It is easy to conceive the extreme fineness that this line must have, since it ought to appear on the space contained between two consecutive divisions on the glass; which spaces are 100dths of millimeters, and so as to permit their evaluation by estimation to 1000dths of a millimeter. I believe it would be in vain to attempt to perform this by placing a wire in the focus; and that the finest of those that are commonly adapted to telescopes, either for the purposes of surveying, or for those of astronomy, cover spaces much too great to allow of a similar estimation, although they only undergo the magnifying power of the eye-glass. I received from M. Breguet, member of the Board of Longitude, a platina wire made in England by an ingenious process. This wire had been passed through a hole when enveloped in a covering of silver; and when the compound of the two metals had been reduced to its greatest fineness, the silver was dissolved, and the platina wire left uncovered. The maker had written on the piece which contained the wire of which I speak the number 6000, to indicate that its diameter is $\frac{1}{6000}$dth of a fraction of an English foot, which M. Breguet thinks is a *line*. If he has been rightly informed in this respect, there is an enormous miscalculation in the evaluation of the maker; for his wire, when stretched and put in contact with the division of 100dths of a millimeter, covered the interval between two strokes, and the strokes themselves. Its magnitude is therefore more than 0·01 millimeter, whilst the English evaluation only makes it 0·00035 millimeter: and if, as I am inclined to believe, it is not 6000dths of a *line*, but only 6,000dths of an inch, that he intended to indicate by the number 6000 written on the side of the wire, there is still an error of 3-5ths; for the $\frac{1}{6000}$dth part of an English inch = 0·0042 millimeter.

This wire, which has probably the greatest degree of fineness that can be attained in the present state of the arts, does not therefore give us the most delicate line that can be rendered perceptible to the eye; and my divisions of 100dths of millimeters on glass serve as a proof of this. The thickness of each line of this division is only about the third of the length of the interval contained between two immediately adjoining strokes; so that this thickness is, according to what I have said above, less than the third of the diameter of the English platina wire. For these reasons I have determined not to put the wire in the focus of the microscope, but to put a piece of plain glass there, on which M. Richer has traced for me two lines at right angles, of such a degree of fineness and neatness, that when one of these lines projects itself between two strokes of the divisions on glass, the proportion between its distances from each adjoining stroke may easily be estimated. This expedient affords also the advantage of great solidity, and that of rendering the application of a vernier to the apparatus quite easy by having on the glass in the focus ten parallel strokes, which should cover 9 or 11 of the divisions of 100dths of a millimeter.

It is unnecessary to trouble ourselves about the loss of light occasioned by this glass in the focus; for notwithstanding its interposition between the eye and the object, an intensity of light that the eye still supports very well, causes the strokes marked on the glass, bearing the divisions into 100dths of a millimeter, to disappear. It has sometimes happened, that I have intercepted a part of this light by placing my hand before the reflecting mirror: and I have remarked, that in seeking by the vertical motion of this hand the proper position for the degree of light that I wanted, I made the point of collimation to vary within the limits of about $\frac{4}{100}$dth of a millimeter. According to this observation it is necessary, whilst we are comparing the linear measures, to keep the quantity of light thrown on the divided glass in the interior of the microscope always in the same state.

The accuracy and convenience of my new *comparer* has already been submitted to frequent trials. The ratios of some of the linear measures on which it has been tried had been determined with the great *comparer* of M. Lenoir, and the agreement between the results furnished by both instruments has been very satisfactory. Among the operations from which these results are derived there is one that I made with my colleagues Messrs. Bouvard and Arago. One of the objects of comparison was the standard platina *metre* of the observatory.

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To Mr. Tilloch.

DEAR SIR,—IN consequence of the observations made in the preceding paper, on the size of the very fine platina wire so ingeniously contrived and made by Dr. Wollaston, I deemed it proper to inform him that I intended to present you with a translation of M. de Prony's Memoir for publication in your valuable Magazine. I have accordingly been favoured by that gentleman with the subjoined information on the subject, which not only explains the difficulty, but informs us of the manner in which the wire is produced, and the mode adopted for estimating its size. The portions of an inch called *lines*, which the French scientific gentlemen use very commonly, are rarely employed in this country: it was therefore very natural to conclude, that M. Breguet must have made some mistake, in the statement of its dimensions which he carried back with him to France, and communicated to M. de Prony.

I remain, dear sir,
Yours, &c. &c.

Christ's Hospital, Feb. 20, 1816. **T. S. EVANS.**

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"When Mr. Breguet was in London, he received from Dr. Wollaston a specimen of platina wire $\frac{1}{3000}$dth of an *inch* in diameter.

"The data from which Dr. W. infers the dimensions of so small a wire may serve as a means of estimating the accuracy of M. Prony's method when applied to the measure of such objects.

"A wire of pure platina is drawn till ten grains of it measure 24 inches, so that its diameter is thus known to be $\frac{1}{100}$dth of an inch.

"A portion of this wire is then coated with silver cast round it in a cylindrical mould, (about $\frac{3}{10}$ths of an inch in diameter).

"The cylinder is then drawn till each inch is elongated to 400 inches, in which state the diameter of the platina is known to be reduced in the proportion of the square root of 400 or twenty-fold: so that its diameter is then $\frac{1}{2000}$dth of an inch.

"If any portion of the silver wire be then further drawn till one inch measures nine inches, the platina wire within it is then reduced to $\frac{1}{3}$d part of its last diameter, and is consequently $\frac{1}{6000}$dth of an inch in thickness.

"If the silver wire be then dissolved by nitric acid, the diameter of the platina which remains undissolved (although kept perfectly clean) could not with confidence be pronounced inaccurate by a mensuration in which its dimensions were at first presumed to be $\frac{1}{18000}$dths of an inch.

Feb. 20, 1816. "W. H. Wollaston."

XXVIII. *Remarks on the Geological Sketch of a Part of Cumberland and Westmoreland.*

To Mr. Tilloch.

Sir,—In your Magazine for last month, (p. 41,) in an article entitled "A Geological Sketch of a Part of Cumberland and Westmoreland," your correspondent notices as a fact, that carbonate of strontites has been found in the basalt of the Giant's Causeway. This is a circumstance that has frequently been noticed to me, and specimens have been shown me, as carbonate of strontites, which I have uniformly found to be carbonate of lime. It is nothing uncommon to mistake the one substance for the other, the carbonate of strontites from Braunsdorf in Saxony having long been considered as the hard carbonate of lime among the German mineralogists.—I should therefore be glad to know from your correspondent, through the medium of your Magazine, whether the specimen he refers to has been submitted to analysis.

Your correspondent likewise notices that the clay-slate and hornblend slate of Skiddaw rest immediately on granite; which would lead one to believe that granite occurred on Skiddaw itself. This is not the case: the nearest occurrence of granite to Skiddaw is at a considerable distance on the east of the mountain, beyond where granite again occurs, on the road between Kendal and Chale. There is a third station in that district of country where granite is also found; on the sea coast, near Muncaster; besides which in Wartdale, as well as at Buttermere, a species of rock occurs, which bears a strong resemblance to some varieties of granite. I found the relations of these two last-mentioned rocks so very obscurely marked, that I cannot pretend to give any opinion about them. I notice the localities, with a view to direct the attention of other geologists. My visits to this beautiful country have been pretty much like those of other tourists; I have therefore had little time to follow up any observations.—I have seldom found any country so very puzzling to make out, or to describe geologically in any thing like a satisfactory manner. To this, therefore, I attribute the neglect it appears to have experienced. It remains a fertile source of geological disquisition, and theorists will find it as difficult to accommodate to their respective opinions as any district between this and Kamschatka.

Yours,

Edinburgh, Feb. 15, 1816. S. N.

XXIX. *New Doctrines, as to the Nature of Mathematical and other Certainty.*

To Mr. Tilloch.

Sir,—Happening a few evenings ago to be in a company of persons supposed to be distinguished by their learning and acquirements, the subject of conversation accidentally turned on the nature of the evidences of different truths or certainties; when the Editor of one of our Monthly Journals being present, he with great earnestness maintained, that there is no kind of superiority in mathematical certainty over that arising from testimony; "and mathematicians (said he) have deceived themselves into a contrary belief, from not being aware that every *two things*, said by them to be demonstrated *to be equal*, are in reality *the same thing!*. When for instance (continued he) they say, the *three angles* of a triangle *are equal* to two right ones, their reasoning only amounts to showing that they are *the same!*" and so on.

One of the company happening now to remark, that this was but to over-look the distinction between and to confound *equality*

lity and *identity*; and looking round and perceiving a suppressed smile in the countenances of some of his auditors, the first speaker resumed, by saying, that he considered *the evidence* of various historical facts, such for instances, that there was such a man as Julius Cæsar, and that there is such a place as Rome, &c. as not less complete and *perfect*, with any mathematical truth whatever!. "Nay (continued he), I have even been *assured by single persons* of some things, I as firmly believe as I do *any truths whatsoever!*"

Perceiving now a more general and stronger tendency to smiling, in the attentive circle around him, the speaker (who is a North Briton) rather hastily concluded, by saying, "I know that in this country such opinions are only laughed at, and that every English mathematician holds a different opinion: but that shall not induce me to change mine:"—and I think I heard him add, while the group which had stood around him was in the act of separating, that he intended shortly to enlarge on these topics in his Journal.

Relying confidently, as I do, on the issue of any investigation into the paramount pretensions of *mathematical*, over almost all, if not every other kind of *certainty* (which latter are indeed but *probabilities*, sometimes of a very high amount it is admitted*), I sincerely hope that the learned Editor will not fail of either supporting or retracting his rather singular opinions, as above mentioned. Whether this may or may not happen to be the case, I judge it proper to request the favour of your insertion of this notice of these opinions; and to request of some more able hand, who heard them advanced, or others, to enter without delay on their refutation in your Magazine, which will oblige,

Yours, &c.

MATHEMATICUS.

XXX. *Speedy and easy Method of copying Drawings.*
By M. DE LASTEYRIE†.

M. DE LASTEYRIE first followed the method of rendering the paper transparent by rubbing it with oil of petroleum or asphaltum. This method completely answered the purpose; but having observed that this oil gave out a strong and disagreeable smell, and that it evaporated too quickly, he conceived that the different essences extracted from vegetables, without being attended with the same inconveniences, might give to paper a sufficient degree of transparency to allow a drawing upon which it might be laid to be distinctly seen through it.

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* The speaker insisted much, on *the absolute certainty* that the sun will rise tomorrow; and was very free in his censures, of what the celebrated Laplace has advanced on this as *a probable* event. Speaking of the *laws of nature*, he said, none ought to be admitted as such which every man does not know and fully admit *without demonstration*; that is, *intuitively!* was added, either by the speaker or some one of his hearers, without any dissent therefrom on his part.

† From the *Archives des Découvertes.*

For this purpose he employed essence of lavender, or oil of spike, and essence of citron, which are preferable to oil of petroleum; because instead of having a bad smell they yield a very agreeable perfume: and as they evaporate more slowly, the paper retains its transparency for a greater length of time, by which means the operation of copying is facilitated.

The essences used for this process ought to be as limpid as the purest water. If they were coloured, they would leave marks and stains upon the paper. They are rarely to be met with in commerce in a state of sufficient purity; but they may be easily freed from their colouring particles by a second distillation.

Take a sheet of common drawing paper, and smear it on one side only with a brush dipped into the essence. The space thus rendered transparent should be about seven or eight decimetres square; for, if it be much larger, the essence may evaporate before the drawing is finished, and the paper lose its transparency.

This done, lay the paper upon the drawing, and trace with the pencil the strokes that are seen underneath. Having copied the drawing upon that part of the paper which has been smeared with the essence, rub it upon the other parts, and thus proceed till the whole is finished. Then hold the paper before the fire, shaking it gently to accelelate the evaporation; taking care that it be at such a distance that the hand can bear the heat without inconvenience. When the evaporation is completed, the paper again becomes opaque, and retains all its original whiteness. You may then draw upon it with a pen, wash or paint with colours.

The process is as easy in practice as it is advantageous, on account of the time which it saves, and the precision and accuracy with which you obtain *fac-similes* of prints and drawings that you wish to copy.

XXXI. *On a Method of making Ship Lanterns with Mica and Wire.* By M. Rochon *.

The great fragility of glass does not allow of its being employed in every sort of lantern, lamp, &c. In the navy it is necessary that the watch-lights, those of the powder-room, &c. should transmit the light through horn, or other substance capable of resisting great shocks. This material is at present sufficiently plentiful, and is very well manufactured, in France: but as it fell short in the magazines at the beginning of the revolution, M. Rochon supplied its place for the ship-lanterns by a net-work of wire, of a large mesh, covered with a light coat of transparent isinglass. This artificial horn was at that time of great service in the navy.

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* From the *Annales des Arts et Manufactures.*

The arrival of an American vessel having on board several pieces of foliated mica perfectly transparent, suggested to M. Rochon the idea of employing it in the place of glass or horn, in preference to isinglass and copal varnish. This mineral is found in abundance in the quarries of granite in the environs of Newport in North America. Hitherto it has been only known in the district of Witten, in Siberia, which furnishes it in large flakes. The preparation of this mineral, to render it of use as a medium for transmitting light, consists in separating it in plates, more or less thick, with a double-edged knife. The Siberians, says the traveller Gmelin, use these transparent plates as much for their windows as for their lanterns. The Russian navy consumes a great deal of it; all the window-lights of the vessels are of this substance, which, independent of its great transparency, resists the strong shock of artillery. The surfaces of the flakes or plates of this incombustible mineral are about two ells square. M. Vauquelin has found in it ten parts of silex, seven of alumine; and the limits of its specific weight are, according to the calculations of M. Brissot, between 265 and 293.

We are assured that the Americans use foliated mica for the same purposes as the Russians. They employ also demispherical masses of glass to reflect the rays of light in such parts of the vessels where the blowing of the wind will not allow of lanterns.

Although we might be able to procure squares of mica sufficiently transparent, and thick enough to resist the most violent blows, the necessity of œconomizing a substance so rare, and to give it the utmost degree of transparency, determined M. Rochon to inclose it between two pieces of tinned iron net-work of a large mesh. The wires of these meshes, which are manufactured in a weaver's loom, do not intercept a hundredth part of the light. By these means he is enabled to make the squares of an unlimited size with plates that are of unequal sizes. Gum arabic serves to connect them together; and with some very fine copper wire, well-tempered, a few stitches made with a fine needle will fit it firmly in the frame that incloses it.

One of the light-houses on the coast of Bretagne, at the entrance of the Channel, having had its windows broken by an accident, which extinguished all the fires during the night, and the magazines of the navy being at that time unprovided with panes of sufficient dimensions, M. Rochon promptly supplied the deficiency, by putting up panes made of the mica of Newport. We see, therefore, that this substance may be rendered useful for such purposes, although it does not appear that the navy have yet adopted it. Its dearness is probably the reason. The same reason too will, doubtless, for a long time, prevent it from being brought into domestic use; and our ordinary lanterns happily at present require no alteration.

XXXII. *Notices respecting New Books.*

Elements of Electricity and Electro-Chemistry. By George John Singer, *Esq.*

[Continued from p. 62.]

The third part of Mr. Singer's work treats of the "natural agencies of electricity," and includes four chapters, arranged as follows:

Chap. I. On the identity of electricity and the cause of lightning.

Chap. II. On the phænomena of thunder-storms, and on the probable sources of atmosphercical electricity.

Chap. III. On some luminous phænomena of the atmosphere, the observations of atmospherical electricity, and the arrangement of a new system of insulation.

Chap. IV. Connexion of electricity with medicine and with natural history.

The utility of this part of the treatise renders it highly interesting; and we observe with pleasure, that Mr. Singer has bestowed particular attention on the most useful applications of electrical science, and that he has given very extensive practical information on this part of his subject. We cannot attempt to convey more than a general idea of the ample details this portion of the work contains. The following is extracted from the description of a lightning conductor, p. 224. "The conducting rod or rods (for if the building is large there should be several) should be formed of copper or iron, three quarters of an inch thick. Its upper extremity should be acutely pointed, and rise three or four feet above the highest part of the building. The parts of which the rod is formed should be joined closely; the ends that are applied to each other being screwed together. All the metallic parts of the roof should be connected with the rod, and it should pass down in as direct a line as possible, and penetrate several feet below the foundation, from which it should be inclined outward. The underground part of the conductor is better formed of copper, to prevent its decay; it should be connected, if it possibly can, with a moist stratum of earth or with a large body of water. The penetration of the conductor to some depth below the level of the foundation, will in many instances procure this advantage for it. The conductor is sometimes made wholly of copper, it may then be thinner than if made of iron: for a stationary conductor, I should conceive that a copper rod of half an inch thick would answer every required purpose; and there is little doubt that a less quantity of metal made into a hollow tube so as to increase its superficies would be equally or even more effectual. Conductors for ships have been made of chains (which are highly improper) and of copper wires which are easily attached: but they are with equal ease detached; and I have been informed by several captains, that in many ships furnished with such conductors, they are kept in an inactive state packed up below during long and hazardous voyages. For this reason it would be better that fixed conductors should be employed; they might I should conceive be attached to the mast, and where motion is required an interruption should be made in the inflexible conductor, and its parts be connected together by a length of spiral wire, which would be at once perfectly continuous, and sufficiently flexible to yield to every necessary movement."

Conductors for ships will be much more effectual if they are connected with a strip of metal surrounding the deck, and continued to the copper-bottom. Carriages which are usually filleted round with metal for ornament, may be rendered very secure if these strips are connected with each other, and continued over every edge, so as to surround the prominences of the carriage completely, a metallic communication being continued from them to the ground. In the open air Mr. Singer advises that shelter be not sought beneath a high tree, or building, but considers a distance of twenty or thirty feet from them as rather an eligible situation. He particularly insists on the necessity of avoiding every approach to large masses of water, and even to the streamlets which may have resulted from a recent shower; for they are all excellent conductors, and likely to determine the direction of the explosion. In a house, the partial conductors which usually determine the course of the discharge are for the most part the appendages of the walls and partitions; the most secure situation, therefore, is the middle of the room; and this may be rendered more so, by standing on a glass-legged stool, a mattress, or even a thick woollen hearth-rug. The middle story is the most secure: for explosions sometimes occur from the earth to the clouds; and many instances are on record in which the lowest story of a building has been the only part which has sustained injury; hence it is absurd to take refuge in a cellar, or cave, &c. Chimneys are excellent conductors, from the soot or charcoal with which they are lined: consequently it is essential to avoid any approach to the fire-place: and the same caution is necessary with respect to gilt furniture, bell wires, and extensive surfaces of metal of every description. In a carriage, the precaution of keeping at some distance from its back and sides is also advisable. The several varieties of the thunder-storm are shown to be closely imitated by a proper use of the electrical apparatus; and the source of the vast accumulations of electricity by which these phenomena are produced, is traced to the circulation of water in the atmosphere: hence thunder occurs most frequently in summer, and storms are most tremendous in all situations where the sun's influence operates to the greatest extent. The exact nature of these natural processes of excitation is at present far from being clearly understood; but the analogy between them and many of our artificial methods is clearly described in the following observation of our author:—"The different electrical state of different parts of the atmosphere, obtains principally in the masses of vapour or clouds that float in it; and the origin of this electricity, as well as the cause of its various changes, is probably to be traced to the mutability of these masses; for it has been seen, that change of form, heating and cooling, friction, and the contact of dissimilar bodies, are the artificial sources of electrical excitation, and the clouds experience in succession the operation of all these causes." The opinions of Volta, Saussure, De Luc, &c. are given with great impartiality, and as fully as is consistent with the nature of the work.

The phenomena of the northern lights, and other luminous appearances of the atmosphere, are also treated at some length; and this section deserves notice for the clearness with which the various facts are stated. The author shows a marked distinction between the larger and smaller meteors. He considers the latter as very analogous to the appearances of electricity; but the larger meteors he regards as incapable of explanation in the present state of our knowledge.

In describing the means of observing atmospherical electricity, an account is given of a very remarkable apparatus for that purpose, which has been recently constructed by our correspondent Mr. Crosse of Broomfield. It is described as "consisting of copper wire, one-sixteenth of an inch thick, stretched and insulated between stout upright masts of from 100 to 110 feet in height. The most unwearied exertion has been employed to give unexampled extent and perfection to this apparatus: the insulated wire has been extended to the extraordinary length of one mile and a quarter; and a variety of ingenious contrivances have been applied to preserve the insulation. But the length of the wire rendered it so liable to injury, and subject to depredation, that it has been found expedient to shorten it to 1800 feet, and until the present time no means have been devised that sufficiently preserve the insulation during a dense fog, or driving snow."

The apparatus has been preserved in constant activity during eighteen months; and a series of observations have been made with it, which afford some very interesting information on the subject of atmospherical electricity. The details which Mr. Singer has given are, however, too extensive for the limits of this notice; and we must refer the reader to the work itself for the information they contain.

In pursuing these inquiries, Mr. Singer has been led to a discovery of the first importance to the practical electrician. It is well known that one of the most perplexing cares attendant on electrical experiments, is the preservation of the insulating parts of the apparatus in a perfectly dry state; and in moist weather this is so far from practicable, that many experiments are from necessity avoided in consequence. Mr. Singer has contrived an arrangement which prevents the free contact of the air with the surface of the insulator, and consequently precludes the deposition of moisture upon it, and preserves it in a proper state for use under nearly every variation of weather. The gold-leaf electrometer constructed on this principle becomes a truly valuable instrument: it retains its electricity for a considerable period; and as it scarcely ever requires to be warmed or wiped, is much more useful, and less liable to be deranged, than in the old construction. The description which is given of this method of insulation is nearly confined to the development of its general principle, and is by no means so extensive as we could have wished; for we deem it one of the most useful discoveries in practical electricity which has come under our notice for some time.

The experiments of Nollet and others on the action of electricity on organized bodies are mentioned as introductory to the practice of medical electricity, which is developed with much skill within very moderate limits: an account of the torpedo and gymnotus, which comprises the most interesting facts that are at present known concerning those animals, precedes a detail of the experiments of Galvani, Volta, and others, to the period at which the latter produced his extraordinary invention of the Voltaic battery. This part of the subject is not extensive, but it appears explicit and impartial. Mr. Singer has given it under the general title of Galvanism, because it originated entirely with the experiments of the learned professor of Bologna. Under the arrangement which is here given to it, Galvanism certainly forms a very proper introduction to the fourth and last part of Mr. Singer's work, which has for its subject "Voltaic electricity." This subject, the most novel, and to many the most interesting before us, is discussed in four chapters under the following titles:

Chap. 1. Structure of the Voltaic apparatus, and nature of its electrical phenomena.

Chap. 2. On the chemical effects of the Voltaic apparatus.

Chap. 3. Extensive agency of the Voltaic apparatus as an instrument of chemical analysis. Its influence in the evolution of light, and the production of heat.

Chap. 4. Sketch of the state of theoretical knowledge in Voltaic electricity. Structure and properties of the electric column. The construction of the various parts of the Voltaic apparatus is described in clear terms, and a particularly full account of the process of cementing the trough is given.

The author prefers the original trough of Cruickshank to that formed on the principle of the *couronne de tasses*, which is at present so much in use. In the former, all the plates being acted on at one surface only, each produces its full proportion of effect, and continues for some time active: in the latter, one surface of the zinc is wasted to very little purpose; the acid is sooner saturated, and its action is consequently more transient. To demonstrate the electrical effects of the Voltaic battery, Mr. Singer employs river water only, as the medium of connexion between his plates, and finds it preferable for this purpose to any acid mixture. "With a series of 50 groups a delicate gold-leaf electrometer will be affected without the aid of the condenser. With 100 pairs the divergence of the gold-leaves is sufficiently distinct; and with a series of 1000 groups even pith balls are made to diverge. In these experiments, a wire proceeding from one extremity of the battery is to be connected with the foot of the electrometer, whilst a wire proceeding from the opposite end is brought to touch its cap. The electricity of the zinc side is always positive; that of the copper side always negative." By employing water in his Voltaic apparatus Mr. Singer has discovered that the powers of that apparatus may be accumulated in an electrical battery, which, when connected with it, will always be charged more highly than the apparatus itself, producing loud sparks, and burning metallic leaves, when the Voltaic series employed to communicate the charge to it has no such power alone. This method of employing the Voltaic apparatus is a very excellent one; for it maintains its action without any renewed attention for months, and probably for years: and as the effect increases with the number of plates employed, Mr. Singer suggests that, by employing 50 or 100,000 plates, a considerable charge might be constantly kept up in an electrical battery, and that at no expense but the first cost of the apparatus; which would be sufficient if formed of plates two inches square.

In noticing the chemical action of the Voltaic apparatus it is remarked, that its operations are very different to those of common electricity, which are usually attended by mechanical action, and rarely by any unequivocal production of heat; whilst in the Voltaic apparatus when no light is evolved an elevation of temperature may be usually observed: and when by its intense action there is a copious evolution of light, heat is produced in a superior degree to that which results from any other process of art. When metals are revived from their solutions by Voltaic electricity, it is noticed as a singular fact, that none but lead, tin, and silver, appear as brilliant metallic vegetations. The various experiments of decomposition and transfer, with the interesting discoveries of Sir H. Davy, are detailed with great accuracy and perspicuity, accompanied by occasional observations of the author which tend considerably to the elucidation of the subject. So much information is indeed included in this part of the work, that we have no wish to lessen the interest it is calculated to excite, by giving a series of abridged extracts from that which should be read in its complete state. Considerable attention is given to the experiments and observations of Dr. Wollaston, Sir H. Davy, and others who have made useful researches on the subject. The facts throughout appear to be stated with great impartiality, and the remarks and experiments of the author evince a very perfect acquaintance with his subject, and habits of research and inquiry which cannot fail to render an essential service to the cause in which they are employed. We are pleased to observe that no attempt is made to add to the various hypotheses that have been advanced to account for the phenomena of Voltaic electricity: the experiments and observations of the author appear rather to support the opinions of Volta; yet no hypothesis is insisted on, but the deficiences of the principal ones that have been proposed are noticed very freely, and with a spirit highly characteristic of the independent feelings of the writer.

The ingenious analysis of the Voltaic apparatus by our valuable correspondent, M. De Luc, is noticed with considerable approbation by Mr. Singer, who has extended the experiments of that venerable philosopher, and confirmed the accuracy of his results. He considers them of the greatest importance, as defining most accurately the really efficient parts of the Voltaic apparatus, and providing a source of perpetual electrical action, which may hereafter prove very highly useful.

Supplement to the Fourth and Fifth Editions of the Encyclopædia Britannica, Volume I. Part I. with a Preliminary Dissertation, "exhibiting a general View of the Progress of metaphysical, ethical, and political Philosophy since the Revival of Letters in Europe. By Dugald Stewart, Esq. F.R.SS. Lond. & Edin., &c. &c." Edinburgh : Constable and Co.

We notice the above most valuable publication, chiefly with a view to call the attention of our readers to the very learned and copious dissertation on the progress of human knowledge, which Professor Dugald Stewart has prefixed.

To follow this elegant writer and profound scholar *seriatim*, through his enumeration of those who have contributed to the advancement of science and philosophy since the revival of letters would far exceed the limits which we have prescribed to ourselves in this department of our work; but we have been so smitten with the eloquent and comprehensive review which Mr. Stewart has given of the philosophical life and writings of the great Bacon, that we presume to think that our readers will consider some extracts from that part of the preliminary dissertation as affording an intellectual treat of rare occurrence.

"The state of science," the author philosophically observes, "towards the close of the sixteenth century presented a field of observation singularly calculated to attract the curiosity and to awaken the genius of Bacon; nor was it the least of his personal advantages, that, as the son of one of Queen Elizabeth's ministers, he had a ready access, wherever he went, to the most enlightened society in Europe. While yet only in the seventeenth year of his age, he was removed by his father from Cambridge to Paris, where it is not to be doubted that the novelty of the literary scene must have largely contributed to cherish the natural liberality and independence of his mind. Sir Joshua Reynolds has remarked, in one of his academical discourses, that 'every seminary of learning is surrounded with an atmosphere of floating knowledge, where every mind may imbibe somewhat congenial to its own original conceptions.' He might have added, with still greater truth, that it is an atmosphere, of which it is more peculiarly salutary for those who have been elsewhere reared to breathe the air. The remark is applicable to higher pursuits than were in the contemplation of this philosophical artist; and it suggests a hint of no inconsiderable value for the education of youth.

"The merits of Bacon, as the father of experimental philosophy, are so universally acknowledged, that it would be superfluous to touch upon them here. The lights which he has struck out in various branches of the philosophy of mind, have been much less attended to; although the whole scope and tenor of his speculations show, that to *this* study his genius was far more strongly and happily turned, than to that of the material world. It was not, as some seem to have imagined, by sagacious anticipations of particular discoveries afterwards to be made in physics, that his writings have had so powerful an influence in accelerating the advancement of that science. In the extent and accuracy of his *physical* knowledge, he was far inferior to many of his predecessors; but he surpassed them all in his knowledge of the laws, the resources, and the limits of the human understanding. The sanguine expectations with which he looked forward to the future, were founded solely on his confidence in the untried *capacities of the mind;* and on a conviction of the possibility of invigorating and guiding, by means of logical rules, those faculties which, in all our researches after truth, are the organs or instruments to be employed. 'Such rules,' as he himself has observed, 'do in some sort equal men's wits, and leave no great advantage or pre-eminence to the perfect and excellent motions of the spirit. To draw a straight line, or to describe a circle, by aim of hand only, there must be a great difference between an unsteady and unpractised hand, and a steady and practised; but to do it by rule or compass it is much alike.'

"Nor is it merely as a logician that Bacon is entitled to notice on the present occasion. It would be difficult to name another writer prior to Locke, whose works are enriched with so many just observations on the intellectual phænomena. Among these, the most valuable relate to the laws of memory, and of imagination; the latter of which subjects he seems to have studied with peculiar care. In one short but beautiful paragraph concerning *poetry* (under which title may be comprehended all the various creations of this faculty) he has exhausted every thing that philosophy and good sense have yet had to offer, on what has been since called the *Beau Ideal*; a topic, which has furnished occasion to so many over-refinements among the French critics, and to so much extravagance and mysticism in the *cloud-capt* metaphysics of the new German school. In considering imagination as connected with the nervous system, more particularly as connected with that species of sympathy to which medical writers have given the name of *imitation,* he has suggested some very important hints, which none of his successors have hitherto prosecuted; and has, at the same time, left an example of cautious inquiry, worthy to be studied by all who may attempt to investigate the laws regulating the union between mind and body. His illustration of the different classes of prejudices incident to human nature, is, in point of practical utility, at least equal to any thing on that head to be found in Locke; of whom it is impossible to forbear remarking, as a circumstance not easily explicable, that he should have resumed this important discussion, without once mentioning the name of his great predecessor. The chief improvement made by Locke, in the further prosecution of the argument, is the application of Hobbes's theory of association, to explain in what manner these prejudices are originally generated.

"In Bacon's scattered hints on topics connected with the philosophy of the mind, strictly so called, nothing is more remarkable than the precise and just ideas they display of the proper aim of this science. He had manifestly reflected much and successfully on the operations of his own understanding, and had studied with uncommon sagacity the intellectual characters of others. Of his reflections and observations on both subjects, he has recorded many important results; and has in general stated them without the slightest reference to any physiological theory concerning their causes, or to any analogical explanations founded on the caprices of metaphorical language. If, on some occasions, he assumes the existence of *animal spirits*, as the medium of communication between soul and body, it must be remembered, that this was *then* the universal belief of the learned; and that it was at a much later period not less confidently avowed by Locke. Nor ought it to be overlooked (I mention it to the credit of *both* authors), that in such instances the *fact* is commonly so stated, as to render it easy for the reader to detach it from the *theory*. As to the scholastic questions concerning the nature and essence of mind,—whether it be extended or unextended? whether it have any relation to space or to time? or whether (as was contended by others) it exist in *every ubi*, but in *no place*?—Bacon has uniformly passed them over with silent contempt; and has probably contributed not less effectually to bring them into general discredit, by this indirect intimation of his own opinion, than if he had descended to the ungrateful task of exposing their absurdity.

"While Bacon, however, so cautiously avoids these unprofitable discussions about the nature of mind, he decidedly states his conviction, that the *faculties* of man differ not merely in degree, but in kind, from the instincts of the brutes. 'I do not, therefore,' he observes on one occasion, 'approve of that confused and promiscuous method in which philosophers are accustomed to treat of pneumatology; as if the human soul ranked above those of brutes, merely like the sun above the stars, or like gold above other metals.'

"Among the various topics started by Bacon for the consideration of future logicians, he did not overlook (what may be justly regarded, in a practical view, as the most interesting of all logical problems) the question concerning the mutual influence of thought and of language on each other. 'Men believe,' says he, 'that their reason governs their words; but it often happens that words have power enough *to re-act upon* reason.' This aphorism may be considered as the text of by far the most valuable part of Locke's Essay,—*that* which relates to the imperfections and abuse of words; but it was not till within the last twenty years that its depth and importance were perceived in all their extent. I need scarcely say, that I allude to the excellent Memoirs of M. Prevost and of M. Degerando, 'On Signs considered in their Connexion with the Intellectual Operations.' The anticipations formed by Bacon, of that branch of modern logic which relates to *universal grammar*, do no less honour to his sagacity. 'Grammar,' he observes, 'is of two kinds, the one literary, the other philosophical. The former has for its object to trace the analogies running through the structure of a particular tongue, so as to facilitate its acquisition to a foreigner, or to enable him to speak it with correctness and purity. The latter directs the attention, *not* to the analogies which words bear to words, but to the analogies which words bear to things;' or, as he afterwards explains himself more clearly, 'to language considered as the sensible portraiture or image of the mental processes.' In further illustration of these hints, he takes notice of the lights which the different genius of different languages reflects on the characters and habits of those by whom they were respectively spoken. 'Thus,' says he, 'it is easy to perceive that the Greeks were addicted to the culture of the arts, the Romans engrossed with the conduct of affairs; inasmuch as the technical distinctions introduced in the progress of refinement require the aid of compounded words; while the real business of life stands in no need of so artificial a phraseology.' Ideas of this sort have, in the course of a very few years, already become common, and almost tritical; but how different was the case two centuries ago!

"With these sound and enlarged views concerning the philosophy of the mind, it will not appear surprising to those who have attended to the slow and irregular advances of human reason, that Bacon should occasionally blend incidental remarks, savouring of the habits of thinking prevalent in his time. A curious example of this occurs in the same chapter which contains his excellent definition or description of universal grammar.

'This too,' he observes, 'is worthy of notice, that the ancient languages were full of declensions, of cases, of conjugations, of tenses, and of other similar inflections; while the modern, almost entirely destitute of these, indolently accomplish the same purpose by the help of prepositions, and of auxiliary verbs. 'Whence,' he continues, 'may be inferred (however we may flatter ourselves with the idea of our own superiority), that the human intellect was much more acute and subtile in ancient than it now is in modern times.' How very unlike is this last reflection to the usual strain of Bacon's writings! It seems, indeed, much more congenial to the philosophy of Mr. Harris and of Lord Monboddo; and it has accordingly been sanctioned with the approbation of both these learned authors. If my memory does not deceive me, it is the only passage in Bacon's works, which Lord Monboddo has anywhere condescended to quote.

"These observations afford me a convenient opportunity for remarking the progress and diffusion of *the philosophical spirit*, since the beginning of the seventeenth century. In the short passage just cited from Bacon, there are involved no less than two capital errors, which are now almost universally ranked, by men of education, among the grossest prejudices of the multitude. The one, that the declensions and conjugations of the ancient languages, and the modern substitution in their place of prepositions and auxiliary verbs, are, both of them, the deliberate and systematical contrivances of speculative grammarians; the other (still less analogous to Bacon's general style of reasoning), that the faculties of man have declined as the world has grown older. Both of these errors may be now said to have disappeared entirely. The latter, more particularly, must, to the rising generation, seem so absurd, that it almost requires an apology to have mentioned it. That the capacities of the human mind have been in all ages the same; and that the diversity of phænomena exhibited by our species is the result merely of the different circumstances in which men are placed, has been long received as an incontrovertible logical maxim; or rather, such is the influence of early instruction, that we are apt to regard it as one of the most obvious suggestions of common sense. And yet, till about the time of Montesquieu, it was by no means so generally recognised by the learned, as to have a sensible influence on the fashionable tone of thinking over Europe. The application of this fundamental and leading idea to the natural or *theoretical history* of society in all its various aspects;—to the history of languages, of the arts, of the sciences, of laws, of government, of manners, and of religion,—is the peculiar glory of the latter half of the eighteenth century; and forms a characteristical feature in its philosophy, which even the imagination of Bacon was unable to foresee.

"It would be endless to particularize the original suggestions thrown out by Bacon on topics connected with the science of mind. The few passages of this sort already quoted, are produced merely as a specimen of the rest. They are by no means selected as the most important in his writings; but, as they happened to be those which had left the strongest impression on my memory, I thought them as likely as any other, to invite the curiosity of my readers to a careful examination of the rich mine from which they are extracted.

"The ethical disquisitions of Bacon are almost entirely of a practical nature. Of the two theoretical questions so much agitated, in both parts of this island, during the eighteenth century, concerning the *principle* and the *object* of moral approbation, he has said nothing; but he has opened some new and interesting views with respect to the influence of *custom* and the formation of *habits*;—a most important article of moral philosophy, on which he has enlarged more ably and more usefully than any writer since Aristotle. Under the same head of *Ethics* may be mentioned the small volume to which he has given the title of *Essays*; the best known and the most popular of all his works. It is also one of those where the superiority of his genius appears to the greatest advantage; the novelty and depth of his reflections often receiving a strong relief from the triteness of his subject. It may be read from beginning to end in a few hours,—and yet, after the twentieth perusal, one seldom fails to remark in it something overlooked before. This, indeed, is a characteristic of all Bacon's writings, and is only to be accounted for by the inexhaustible aliment they furnish to our own thoughts, and the sympathetic activity they impart to our torpid faculties.

"The suggestions of Bacon for the improvement of political philosophy, exhibit as strong a contrast to the narrow systems of contemporary statesmen, as the inductive logic to that of the schools. How profound and comprehensive are the views opened in the following passages, when compared with the scope of the celebrated treatise *De Jure Belli et Pacis!* a work which was first published about a year before Bacon's death, and which continued, for a hundred and fifty years afterwards, to be regarded in all the protestant universities of Europe as an inexhaustible treasure of moral and jurisprudential wisdom!

"'The ultimate object which legislators ought to have in view, and to which all their enactments and sanctions ought to be subservient, is, *that the citizens may live happily.* For this purpose, it is necessary that they should receive a religious and pious education; that they should be trained to good morals;

that they should be secured from foreign enemies by proper military arrangements; that they should be guarded by an effectual police against seditions and private injuries; that they should be loyal to government, and obedient to magistrates; and finally, that they should abound in wealth, and in other national resources.'—'The science of such matters certainly belongs more particularly to the province of men who, by habits of public business, have been led to take a comprehensive survey of the social order; of the interests of the community at large; of the rules of natural equity; of the manners of nations: of the different forms of government; and who are thus prepared to reason concerning the wisdom of laws, both from considerations of justice and of policy. The great desideratum, accordingly, is, by investigating the principles of *natural justice*, and those of *political expediency*, to exhibit a theoretical model of legislation, which, while it serves as a standard for estimating the comparative excellence of municipal codes, may suggest hints for their correction and improvement, to such as have at heart the welfare of mankind.'

"How precise the notion was that Bacon had formed of a philosophical system of jurisprudence (with which as a standard the municipal laws of different nations might be compared), appears from a remarkable expression, in which he mentions it as the proper business of those who might attempt to carry his plan into execution, to investigate those '*leges legum*, ex quibus informatio peti possit, quid in singulis legibus bene aut perperam positum aut constitutum sit.' I do not know if, in Bacon's prophetic anticipations of the future progress of physics, there be anything more characteristical, both of the grandeur and of the justness of his conceptions, than this short definition; more particularly, when we consider how widely Grotius, in a work professedly devoted to this very inquiry, was soon after to wander from the right path, in consequence of his vague and wavering idea of the aim of his researches.

"The sagacity, however, displayed in these, and various other passages of a similar import, can by no means be duly appreciated, without attending, at the same time, to the cautious and temperate maxims so frequently inculcated by the author on the subject of political innovation. 'A stubborn retention of customs is a turbulent thing, not less than the introduction of new.'—'Time is the greatest innovator; shall we then not imitate time, which innovates so silently as to mock the sense?' Nearly connected with these aphorisms, are the profound reflections in the first book *De Augmentis Scientiarum*, on the necessity of accommodating every new institution to the character and circumstances of the people for whom it is intended; and on the peculiar danger which literary men run of overlooking this consideration, from the familiar acquaintance they acquire, in the course of their early studies, with the ideas and sentiments of the ancient classics.

" The remark of Bacon on the systematical policy of Henry VII. was manifestly suggested by the same train of thinking. ' His laws (whoso marks them well) were deep and not vulgar; not made on the spur of a particular occasion for the present, but out of providence for the future; to make the estate of his people still more and more happy, after the manner of the legislators in ancient and heroic times.' How far this noble eulogy was merited, either by the legislators of antiquity, or by the modern prince on whom Bacon has bestowed it, is a question of little moment. I quote it merely on account of the important philosophical distinction which it indirectly marks, between ' deep and vulgar laws;' the former invariably aiming to accomplish their end, not by giving any sudden shock to the feelings and interests of the existing generation, but by allowing to natural causes time and opportunity to operate; and by removing those artificial obstacles which check the progressive tendencies of society. It is probable that, on this occasion, Bacon had an eye more particularly to the memorable *statute of alienation;* to the effects of which (whatever were the motives of its author) the above description certainly applies in an eminent degree.

" After all, however, it must be acknowledged, that it is rather in his general views and maxims, than in the details of his political theories, that Bacon's sagacity appears to advantage. His notions with respect to commercial policy seem to have been more peculiarly erroneous; originating in an overweening opinion of the efficacy of law, in matters where natural causes ought to be allowed a free operation. It is observed by Mr. Hume, that the statutes of Henry VII. relating to the police of his kingdom, are generally contrived with more judgement than his commercial regulations. The same writer adds, that ' the more simple ideas of order and equity are sufficient to guide a legislator in everything that regards the internal administration of justice; but that the principles of commerce are much more complicated, and require long experience and deep reflection to be well understood in any state. The real consequence is *there* often contrary to first appearances. No wonder that during the reign of Henry VII. these matters were frequently mistaken; and it may safely be affirmed, that even in the age of Lord Bacon very imperfect and erroneous ideas were formed on that subject.'

" The instances mentioned by Hume in confirmation of these general remarks, are peculiarly gratifying to those who have a pleasure in tracing the slow but certain progress of reason and liberality. 'During the reign,' says he, 'of Henry VII. it was prohibited to export horses, as if that exportation did not encourage the breed, and make them more plentiful in the kingdom. Prices were also affixed to woollen cloths, to caps and hats, and the wages of labourers were regulated by law. It is evident *that these matters ought always to be left free, and be intrusted to the common course of business and commerce.*'— 'For a like reason,' the historian continues, 'the law enacted against inclosures, and for the keeping up of farm-houses, scarcely deserves the praises bestowed on it by Lord Bacon. If husbandmen understand agriculture, and have a ready vent for their commodities, we need not dread a diminution of the people employed in the country. During a century and a half after this period, there was a frequent renewal of laws and edicts against depopulation; whence we may infer, that none of them were ever executed. *The natural course of improvement at last provided a remedy.*'

"These acute and decisive strictures on the impolicy of some laws highly applauded by Bacon, while they strongly illustrate the narrow and mistaken views in political œconomy entertained by the wisest statesmen and philosophers two centuries ago, afford at the same time a proof of the general diffusion which has since taken place among the people of Great Britain, of juster and more enlightened opinions on this important branch of legislation. Wherever such doctrines find their way into the page of history, it may be safely inferred that the public mind is not indisposed to give them a welcome reception.

"The ideas of Bacon concerning the education of youth, were such as might be expected from a philosophical statesman. On the conduct of education in general, with a view to the development and improvement of the intellectual character, he has suggested various useful hints in different parts of his works; but what I wish chiefly to remark at present is, the paramount importance which he has attached to the education of the people,—comparing (as he has repeatedly done) the effects of early culture on the understanding and the heart, to the abundant harvest which rewards the diligent husbandman for the toils of the spring. To this analogy he seems to have been particularly anxious to attract the attention of his readers, by bestowing on education the title of *the Georgics of the mind;* identifying, by a happy and impressive metaphor, the two proudest functions intrusted to the legislator,—the encouragement of agricultural industry, and the care of national instruction. In both instances, the legislator exerts a power which is literally *productive* or *creative;* compelling, in the one case, the unprofitable desert to pour fourth its latent riches; and in the other, vivifying the dormant seeds of genius and virtue, and redeeming from the neglected wastes of human intellect, a new and unexpected accession to the common inheritance of mankind.

"When from such speculations as these we descend to the treatise *De Jure Belli et Pacis*, the contrast is mortifying indeed. And yet, so much better suited were the talents and accomplishments of Grotius to the taste not only of his contemporaries but of their remote descendants, that, while the merits of Bacon failed, for a century and a half, to command the general admiration of Europe, Grotius continued, even in our British universities, the acknowledged oracle of jurisprudence and of ethics, till long after the death of Montesquieu. Nor was Bacon himself unapprized of the slow growth of his posthumous fame. No writer seems ever to have felt more deeply, that he properly belonged to a later and more enlightened age;—a sentiment which he has pathetically expressed in that clause of his testament, where he "bequeaths his name to posterity, after some generations shall be past."

Mr. Accum has in the press *a third edition* of his Practical Treatise on Gas Light. Exhibiting a summary description of the apparatus and machinery best calculated for illuminating streets, houses, and manufactories, with coal-gas. With remarks on the utility, safety, and general nature of this new branch of civil œconomy. The work will be published the 1st of March.

In the press. A new Work entitled "The Elements and Genius of the French Language, being a natural and rational method of teaching a language with sentences deduced from the analysis of the human mind."

"Memoirs of the Ionian Isles, and of their Relations with European Turkey; translated from the original Manuscript of M. de Vaudoncourt, late General in the Italian service: with a very accurate and comprehensive Map."

XXXIII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

Feb. 1 and 8. On these evenings the conclusion of Dr. Wilson *Phillip's* experiments to ascertain the relation between the sanguiferous and nervous systems, and the ganglia, was read. This paper, which is the third and concluding one on this curious subject, entered into a very wide field of physiological research, and embraced many singular conclusions and general inferences, which cannot be adequately detailed in this abstract. The author states, that the sanguiferous system can exist independent of the nerves; but the latter can stimulate the former, or retard and even totally obstruct it. He next took an extensive view of secretion, as connected with these systems, the ganglia and spinal marrow. Animal heat he considers a secretion. When the fluids secreted by the glands are accumulated, secretion is not therefore suspended; on the contrary, its continuance is necessary to the healthy state of the glands and the other vital functions. Secretion and galvanism he thinks produce similar effects. In conclusion the author observed, that in all his experiments he had, wherever it was practicable without injury to the result, destroyed the sensibility of the animal previous to the commencement of his operations, and had also avoided all unnecessary repetition of cruel experiments, or any useless waste of animal life. He proceeded to lay down some general and very judicious rules to avoid cruelty in making such experiments, and reflected on the conduct of some French physiologists in this respect. The object, he observed, of such operations is the ultimate advantage of society: if that be obtainable, it was weakness, and not humanity, to reject or decline them; if not, it would be wanton cruelty.

A letter from Dr. Brewster to the right honourable President was read, relating some new experiments on the double refractive powers of fluate of lime and muriate of soda. Hauy had justly observed, that all crystals have regular cubes, or tetrahedrons; for their integral molecules are devoid of the property of double refracting crystals. Malus and Biot confirmed this observation; but Dr. B. has discovered, that under peculiar circumstances, fluate of lime and muriate of soda have a double refractive power, and polarize light. He found that a mass of fluate of lime, having a cube in the centre, but surrounded with different facets, polarized light; but that the light passed through the cubic crystal without any change. Muriate of soda, some crystals of which measured three inches, gave very fine colours; blue with complement of red, red with complement of yellow, &c. This discovery not only contributes to confirm the accuracy of crystallography, but will also facilitate its progress, as it must tempt many persons to direct their attention to a branch of science so fertile, and so curious in its phænomena.

Feb. 15. Two mathematical papers were laid before the Society by professor Robertson, F.R.S. but they were of a nature not proper for general reading.

Mr. Todd, a surgeon in the Royal Navy, presented an account of his observations made on the torpedo at the Cape of Good Hope. The peculiar organs of this animal have been described by the late Mr Hunter. Mr. T. found that when the electric organs are often excited they lose their power, and the animal dies much sooner. Its first strokes are always the most violent; and grow gradually more and more feeble until quite exhausted, and then the animal dies. The author cut open the little tubes or electric organs in the breast; and by this process the animal lost its electric powers, but continued to live longer than those whose electricity was entirely exhausted. The torpedos subjected to these experiments were smaller than those found in the northern seas, being only from five to eight inches long, and from three to five broad. They were caught by the sailors when fishing in the usual manner while the Lion lay at anchor off the Cape. Some of the torpedos manifested a kind of reluctance to give shocks; others parted with them very freely: hence the author is inclined to believe that it requires a considerable effort in the animal to give shocks, and one which shortens its life. The torpedos were kept in casks of salt-water, in which they lived from two to five days.

Feb. 21. A short paper by Sir Everard Home was read, containing some observations on the structure of the feet of some lacertæ, particularly the *gecko*. Sir Joseph Banks, who suffered nothing to escape his observation, noticed, while in Batavia, that the gecko is a very familiar inmate of the houses; and that it could run along their smooth cielings, having its back downwards, with the greatest ease, contrary to the laws of gravity. He mentioned this circumstance to Sir Everard, and also supplied him with a large one weighing three ounces, in order that he might examine the structure of its feet The result of his inquiry is, that the feet of the gecko have some resemblance to the *actinia* of those fish which adhere to the sides of ships; that they, at every step, form a partial vaccuum below them, which thus enables them to run with their back downwards.

This evening their imperial highnesses the archdukes John and Lewis of Austria, brothers of the emperor of Austria, having been elected at a previous meeting, were regularly introduced as fellows of the Royal Society of London for improving natural knowledge.

KIRWANIAN SOCIETY OF DUBLIN.

Dec. 13. 1815. A paper "On some liquid Combinations of oxymuriatic Acid, and their Application to the discharging of Turkey Red in Calico Printing," was read by D. Wilson, esq.

Feb. 7, 1816. A paper "On a new Process for obtaining pure Silver, with Observations on the Defects of the Processes hitherto employed," was read by M. Donovan, Esq. Secretary.

We forbear giving any futher account of this paper for the present, as we shall probably be enabled to give it in full in the next month.

Feb. 21. A ballot having taken place, the following gentlemen were elected officers for the current year:

President.

Right Hon. George Knox, M. R. I. A.

Vice Presidents.

J. Ogilby, M.D. | R. Blake, M.D. M.R.I.A.

Secretary and Treasurer.

M. Donovan, Esq.

Council.

S. Witter, Esq. | A Carmichael, Esq. M.R.I.A.
D. Wilson, Esq. | J. Tardy, Esq.
J. Patten, Esq.

XXXIV. *Intelligence and Miscellaneous Articles.*

THE PLANISPHERE OF DENDERA.

In our xivth volume we inserted a copy of the zodiac found in the portico of this temple, accompanied with a dissertation thereon by the now deceased Dr. Henley. The object of the author was, in opposition to the French sceptics, who affected to draw from the structure of this curious astronomical monument an inference that it was at least 17000 years old, to show from evidence furnished by the zodiac itself, that it was not constructed earlier than the reign of Augustus. It would appear that the reasoning of Dr. Henley has been considered as conclusive by the learned, for we have seen no attempt made to controvert it.

Our astronomical readers will be gratified by receiving in the present number a plate representing a planisphere found in one of the apartments of the same temple, most exquisitely engraved by Cardon *.

Our readers will naturally turn to our xivth volume to examine whether any or what coincidence there may be between this planisphere and the zodiac referred to. We shall be happy to receive from any of our correspondents whatever rational conjectures may occur to them respecting the planisphere. To us it appears that it has reference to a period at which *Taurus*, *Leo*, *Scorpio*, and *Aquarius*, were the cardinal signs; for these signs are made to coincide respectively with the four corners of the apartment: but this settles nothing as to the time at which it was set up. We shall only throw out the suggestion (which may yet be very foolish), whether something relative to the precession of the equinoxes is not indicated by the overlapping, as it were, of some of the signs? We are aware that the *duplicates* of *Taurus* and of *Leo* have been considered as extra-zodiacal asterisms; but may they not be intended to mark a difference between the moveable and fixed zodiacs?—between the ideal zodiac, which astronomers always make to commence with the vernal equinox, still calling the first 30° *Aries*, and the stellar constellation originally so called? If so, *Taurus* seems to be marked as the vernal equinox at a period much more ancient than the date of the erection of the temple of Dendera; and if so, by the motion of *Taurus* in the moveable zodiac, when the equinox had changed a whole sign the moveable *Gemini* would come to the place of the stationary *Taurus*, and so of the other signs, making *Aries* to become the vernal sign: and by a second change of a whole sign the moveable *Taurus* would reach the stationary *Pisces*, bringing the moveable *Libra* to the stationary *Leo*. Is this indicated in the planisphere before us? If so, the vernal equinox at the time when this planisphere was put up appears to have been in the stellar *Pisces*, which it is now quitting. The precession of the equinoxes, as it is called, is at the rate of about 21½ centuries for one sign.

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* For the use of this valuable engraving we are indebted to a circumstance which ought not to be concealed. Examining one day at Mr. Taylor's the numbers already published of the work entitled "EGYPT," (noticed in our last,) the sight of this planisphere brought on some conversation relative to the progress which the Egyptians must have made in astronomy. The

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Letter from M. Van Mons, dated Brussels, Feb. 10.

"It is said that Berzelius has made some decisive experiments on chlorine, and on the quantity of oxygen which it contains. I think that after mine his proof was not necessary: and what in fact could we wish for stronger than sweet mercury, which in 100 of oxidule which it contains, gives four of oxygen and corrosive sublimate; which on the same quantity of oxide

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The zodiac of the portico, and the dissertation of Dr Henley, of course were mentioned; and Mr. Taylor, remarking that the plate of the planisphere was so well engraved, that it could bear printing off a sufficient number for the Philosophical Magazine, as well as for the expensive work which he is now publishing, added that he should be pleased to see it in the work in which Dr. Henley's Dissertation had appeared, as it might then meet the eyes of some who have already paid attention to the subject.

gives eight of oxygen; and which leave as their residues, the former a black powder composed of 14 of dry muriatic acid and of 82 of reduced mercury; and the latter a red powder composed of 20 of dry muriatic acid and 72 of reduced mercury; and all powders of astonishing lightness, of dull instead of bright colours, which the oxygen regenerates into their primitive salts, and from which water takes up the dry acids, the mercury remaining reduced?

"Berzelius is of my way of thinking, as to the existence of oxygen in azote. I have subjoined a note on this subject. I was the first to class azote among the combustibles, and the first also to class it among the oxidated bodies. Wherefore all this noise about a new theory?—is it not merely putting in a new form what others have invented?

"Prussic acid has been found in opium in Germany; and from this discovery it has been concluded that the narcotic virtue of opium depends on that acid. You will agree with me, that there is only a step between them.

"I have found that we may extract the soluble parts of most organized substances, by treating their powder precisely like coffee. But if too much boiling water is poured in, nothing more of the substance is communicated to it. I made my first experiment of this kind on gall-nuts in powder, for making ink, and I made it with perfect ease. The ink obtained was a true black, did not become thick, and did not deposit any of its ingredients; but we must use the sulphate oxidated, and not that which is oxidulated, and put in the gum and sugar-candy last of all.

"It is said that Gay-Lussac has published a work on the prussic acid, and that he has found, like me, that this acid is azoto-carbonated hydrogen. I have said, in my translation of Bucholz and of Davy, that this acid is similar to the hydrothionic acid, or sulphuretted hydrogen. The hydrogens proper to the two combustibles, and those which constitute them, azote in the state of ammonia. and sulphur in the state of hydrogenated sulphur, are taken up by the third hydrogen, and the dry acids are exposed. The hydrogens may by heat be taken from the double radical of the prussic acid, without the elements of this radical being separated; and I am led to think that they are united in the same manner as carbon is with sulphur in the alcohol of Lampadius; *i.e.* the combustible which is found in the highest ratio is substituted for the oxygen of the dry acid of the other combustible, this oxygen being proportioned to the water. In the combination the carbon or the azote is in a reduced state, and they are also subsaturated with the quantity of hydrogen which serves to compose their oxygen into water. This explains

plains the reason why the oxygen alone does not decompose the alcohol of Lampadius into acids of its two combustibles; and also shows why the water resolves them into sulphur, and into regenerated carbon. It ought to be the same with the radical of the prussic acid.

"It seems to result from some first experiments of Vauquelin on the hyperoxygenated muriatic acid, that the dry muriatic acid may be saturated with oxygen at least in three ratios.

"The venerable Von Crell, loaded with scientific honours, and full of years, has still had courage and strength of mind enough to translate Thenard's Chemistry.

"It is a remarkable peculiarity, that the father of modern chemists should translate into his own language the work of the youngest. He has enriched his translation with a great number of notes.

"My journal cannot yet make its appearance. This country suffers so much, we are all so miserable and so poor, that I cannot depend on many subscribers; and the misery, the state of disquietude, and want of encouragement, are as general in France.

"I am, &c. J. B. Van Mons."

Note alluded to as subjoined to the above Letter by M. Van Mons.

"M. Berzelius no longer considers azote as the radical elementary combustible of the nitric acid. He thinks that this acid is composed of 88,29 parts of oxygen, and 11,72 parts of an unknown radical, which he calls *nitria*.

"As in 100 parts of nitric acid we find 26,425 of azote only, and 73,575 of oxygen, and as there ought to be 88,29 parts, the 24,715 parts of oxygen wanting must be contained in the 26,425 parts of azote; and this azote ought consequently to be composed of 24,715 of oxygen, and 11,72 of the unknown radical which the author calls *nitric*.

"According to this, 100 parts of azote consist of 44,32 of nitria, and 55,68 of oxygen. The hydrogen acted in these experiments as an elementary body, or at least as a combustible exempt from oxygen.

"If we are to consider hydrogen as an elementary substance, and azote, on the contrary, as an oxide, then ammonia must necessarily have for its principles, nitria, hydrogen, and oxygen. We may always regard it as the oxide of a compound radical. We may figure to ourselves that it consists, according to given volumes, of one proportion of nitria and six proportions of hydrogen:—ammonia, on the contrary, consists of one proportion of nitric, one proportion of oxygen, and six proportions of hydrogen.

“ Berzelius also thinks that the amalgam obtained from the reduction of ammonia contains a peculiar metal. This metal, he observes, cannot be a simple body, since the radical of the ammonia is compound.”

M. Freysmuth has discovered columbium in Bohemia. He extracted it from a mineral which had been hitherto regarded as a native sulphuret of zinc, and also for *nigrin*, which it resembles. The author thinks that tantalium and columbium are very different metals, notwithstanding the resemblances which Wollaston found between them. M. Jacquin has in his possession a piece of columbium extracted from this ore.

M. Michellotti has dissolved silver in the simple muriatic acid at a boiling heat, and obtained a crystallizable salt soluble in water and in alcohol, and which is sublimated after the manner of the muriates called the *butyraceous* muriates. This gentleman thinks that it is a hydrochloride-oxide, the muriate less precipitated being an oxidule. The water is divided more or into a portion with excess of acid, and into a portion with excess of oxide.

MANUSCRIPTS OF HERCULANEUM, AND THE ANTIQUITIES OF POMPEIA.

We copy from a German journal the following note, which contains some facts not perfectly known hitherto, though not of very recent date.

“ On the 26th of November, 1813, a letter was read in the Royal Society of Copenhagen from Mr. Schubart, containing several literary notices from Italy. He relates among other things that about 300 of the Herculanean manuscripts have been unrolled. Among them are the following important works:—

1. Philomedes (should this be Philodemus ?) on the Influence of Music on the human Constitution.
2. Epicurus upon Nature, two volumes.
3. Philomedes on Rhetoric, two parts.
4. Philomedes on the Affinity between the Virtues and the Vices.
5. Philomedes on the Vices.
6. Philomedes on the Poets.
7. Philomedes' Philosophical Fragments.
8. Democritus Geometricus's Fragments.
9. Philostratus on unreasonable Contempt.
10. Carneseus upon Friendship.
11. Cotothes upon Plato's Dialogue of Isis.
12. Philomedes on Religion.
13. Chrysippus on Providence.

"The excavations at Pompeia are prosecuted after a certain plan, so as to go round the whole town, which, when cleared from the ashes that cover it, will probably become one day the most remarkable monument of antiquity."

An improved Printing-machine just completed by Mr. Kœnig, the inventor of that which has been employed upwards of a twelvemonth in printing the *Times* and *Evening Mail* newspapers, was tried for the first time on Wednesday, Feb. 28th, at the manufactory, in the presence of their Imperial Highnesses the Archdukes JOHN and LEWIS. Mr. Kœnig has now made some very material improvements in his invention, by which its advantages are greatly increased.

To Mr. Tilloch.

SIR,—By giving place in your next Magazine to the following, you will oblige many of your scientific friends, as well as a constant reader of your Magazine.

"Lately died at Bristol Mr. Joseph Herbert, an honorary member of the Geological Society, whose assiduity and knowledge in that science were universally acknowledged by those who have witnessed his efforts, and seen his beautiful cabinet of minerals, fossils, shells, &c. the collection of twenty years, and which is to be disposed of by application to Mr. Richard Vigor, No. 2, James's Place, Kingsdown, near Bristol.

"I remain, sir, your obedient servant,

Bristol, Feb. 14. 1816. "RICHARD VIGOR."

To Mr. Tilloch.

SIR,—The following particulars relative to the late intense cold may be worth recording in your Magazine. On the morning of the 7th of February the cold very much increased, and in the night was very severe; the thermometer being about ten degrees of Fahrenheit. On the 8th it scarcely rose above that degree all day, and at midnight was 6°; at three o'clock in the morning of the 9th, 5°; and at sunrise, 4°. In other parts of the parish of Hackney it is said to have been two degrees lower. At Walthamstow, about three the same morning, it was $1\frac{1}{2}$°. The same night it was about $2\frac{1}{2}$° at Clapton. On the 10th it rose several degrees, and the ordinary cold of winter returned. Nothing remarkable either in the height or variation of the barometer, or of any other metercological instrument, occurred; the sky was, during the cold, almost entirely free from clouds. Any observations of other correspondents will oblige Yours, &c.

THOMAS FORSTER.

LECTURE.

Mr. Clarke will commence his next Course of Lectures on Midwifery and the Diseases of Women and Children, on Monday, March 18th. The Lectures are read every Morning from a Quarter past Ten to a Quarter past Eleven, for the convenience of Students attending the Hospitals.

For Particulars apply to Mr. Clarke, at the Lecture-Room, 10, Saville Row, Burlington Gardens.

LIST OF PATENTS FOR NEW INVENTIONS.

To Davis Redmund, of Johnson's-Court, Fleet-Street, London, for his improved machine for the manufacture of corks and bungs.—9th Dec. 1815.—6 months.

To John George Drake, of Chapman-Street, Pentonville, chemist, for a certain method of expelling the molasses of syrup out of refined sugars in a shorter period than is at present practised with pipe-clay.—3d Feb. 1816.—6 months.

To John Millington, of Duke-Street, Manchester-Square, engineer, for certain machinery to be moved by wind, steam, manual labour, or any of the processes now employed for moving machinery by means of which boats, barges, and other floating vessels may be propelled or moved in the water.—1st Feb.—6 months.

To John Budgeon, of Dartford, in the county of Kent, paper-maker, for a process for reducing rags or articles composed of silk or cotton after they have been used, and bringing them into their original state, and rendering the material of which they are composed, fit to be manufactured, and again applied to beneficial and useful purposes.—3d Feb.—2 months.

To John Thomas Dawes, of West Bromwich, in the county of Stafford, ironmaster, for certain improvements in steam-engines, some of which improvements are applicable to other purposes.—6th Feb.—2 months.

To Joseph Barker, of Cottage Green, Camberwell, artist, for certain means of continuing the motion of machinery.—6th Feb. —6 months.

To William Milton, of Heckfield, Hants, clerk, for certain improvements upon the wheels and perches of carriages.—10th Feb.—6 months.

To Henry De Sarul, of Leicester-Street, Leicester-Square, artificial florist, in consequence of a communication made to him by a certain foreigner residing abroad, for his improved cylindrical gold and silver sweep and washing machine.—20th Feb.—6 months.

To William Baynham, of London Road, Surrey, chemist, for his composition of making leather and other articles water-proof.—20th Feb.—6 months.

METEOROLOGICAL TABLE,

BY MR. CARY, OF THE STRAND,

For February 1816.

Days of Month.		Thermometer. 8 o'Clock, Morning.	Noon.	11 o'Clock Night.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
Jan.	27	39	42	33	29·65	[illegible]	Cloudy
	28	30	35	32	30·18	[illegible]	Fair
	29	28	34	26	·36	0	Foggy
	30	26	32	25	·40	10	Fair
	31	23	35	26	·20	12	Fair
Feb.	1	26	30	27	29·80	10	Fair
	2	27	38	42	·58	7	Cloudy
	3	42	47	43	·50	0	Cloudy
	4	42	44	40	·48	0	Rain
	5	42	42	40	·38	0	Cloudy
	6	40	40	32	28·95	0	Rain
	7	32	30	26	·99	0	Snow
	8	20	28	18	29 50	6	Fair
	9	13	25	18	·58	0	Fair
	10	12	29	24	·62	0	Fair
	11	26	36	26	·85	0	Cloudy
	12	24	32	25	30·30	10	Fair
	13	21	36	29	·32	9	Fair
	14	29	37	32	·30	7	Cloudy
	15	36	42	40	·18	10	Fair
	16	39	45	38	29·62	5	Cloudy
	17	34	39	34	·86	6	Fair
	18	27	34	40	30·09	0	Cloudy
	19	40	45	40	29·99	0	Cloudy
	20	40	47	40	·95	10	Fair
	21	40	46	40	30 03	15	Fair
	22	40	46	40	·18	22	Fair
	23	45	52	39	·25	24	Fair
	24	38	54	42	·10	27	Fair
	25	46	55	45	.05	0	Showery

N.B. The Barometer's height is taken at one o'clock.

XXXV. *On a correct practical Method for cutting Spherical Brick Niches—On the Spiral Line—and On Spandrel Groins. By Mr.* RICHARD BROWN, *of Wells-Street, Oxford-Road.*

I. *On Spherical Brick Niches.*

FROM my never having seen in any publication hitherto, a correct practical method for cutting spherical brick niches (a thing so essential) I was induced to send for insertion in your useful Magazine the accompanying plate. It contains an accurate drawing of the moulds and templets as used for gauging the bricks in the head of the niches recently executed in the north flank of Drury Lane Theatre: therefore these lines may safely be depended on when applied to all such future works.

I am, sir,

Yours respectfully,

February 1816. RICHARD BROWN.

On Spherical Niches, Plate II. *fig.* A.

First describe the curvature of the plan with the radii st, sv, then mark the bricks on it, beginning at the front with a header as t, and then with a stretcher as l, and so on alternately. The plan being now laid down, next project the orthographical elevation gg from the plan, and from thence proceed with the divisions of the bricks round the head of the niche, observing that the joints must always be so arranged as to have a brick fall directly over the centre of the niche, as k: this is called the key of the arch. The bricks are next radiated to the centre of the niche, and the heading joints described concentric to each other, which completes the construction of the niche. Next for the moulds:—First form the centring, as shown by the orthographical elevation qhq (which is sufficiently explicit without detail); then from qq bring down the lines to [illegible] and on r with the radius rn again describe the plan of the niche on which are shown the ichnographical appearance of the centring and some of the radiating lines of the brick-work. These lines, which are to be the guide in laying the bricks on the centring, are projected ichnographically on the centring, in the following manner: First, describe the arch $m5$, &c. equal to the rise of the niche head; and for plainer inspection, suppose the arch to contain but five bricks round the head, which arch is here divided into five equal parts, as shown by the numerical figures: next divide half the plan into four equal parts or more (at discretion): these four divisions are then to be drawn parallel to rn, and from 1 2 3 4, lines are to be drawn up to the line 5 *m*, and from thence described round to the radial lines 4, 3. If these lines are now brought down to intersect the parallel lines on the ichnography, the intersections will then give the points through which the curves must pass, as seen described on the centring. These lines being now shown ichnographically on the centring, it is next required to make a mould for drawing them mechanically on the centring, and for gauging the edge of the templet *c,c,c,i*: this is done most expeditiously (and sufficiently accurate for the workman) in the following manner: Take the four numerical divisions from the plan and set them on the line 1 2 3 4 at fig. B; these lines then draw parallel to *qq*, and bring up from the plan of the centring the corresponding lines *ee*, &c. and where they intersect the parallel lines 1 2 3 4 will be the extreme edge of the mould. The other edge of the mould is found by merely transferring the distances on the opposite side of the centre line. The rule *f* shows a mechanical method, as used by some workmen for striking the joints on the centring; but as it is liable to variation, I shall merely explain it: A rule is first fixed at the centre *f* and in the direction of any joint divided round the arch; a string is then fixed at the back of the centring opposite *j*, and the other end of the string brought to the upper edge of the rule, and from that slided gradually down on the centring in the direction of the rule. The joint is then marked by the line with a pencil.

I now proceed with the moulds and templets for gauging and rubbing the bricks by; *dddd* represents the ground (glued up like a piece of dado) to which the inclined wedge-like mould *ccc* is fixed. This mould requires to be very accurately made, as it becomes the regulating mould of all the bricks throughout the niche, and to which the bricks are fitted; *aa* shows the section of the hither end of the mould, which ends imperceptibly in a thin edge at [illegible]wise inclining to *p*, where it will also fall to a point: the edge *ccc* also curving round agreeably to the mould *wx* shown above, and swelled by the mould, fig. B, being bent round it. The mould *aa*, observe, must be the exact thickness of one of the bricks taken from the elevation, as *g*, *o*, for instance; but here the mould is shown as if the bricks were six times as large. This is to make it more evident.

The mould being now made and screwed on the grounds, next mark the divisions of the bricks round the edge of the mould *ccc*, likewise on the grounds, which divisions draw towards *p* by means of a rule. This will now give the mould for the thickness, inclination and joint, of every required brick, and *llll*, taken from *g*, *o*, 7, *o*, &c. every templet throughout the niche, the core excepted.—The moulds *zzz*, fig. C, give the bevel, curvature, and convexity of the bricks in the core of the niche, which core is generally composed of about two bricks, (but sometimes of stone), and is always introduced on account of the bricks becoming so very thin at the back. Thus is the whole sufficiently and practically explained.

II. *On the Spiral Line.*

The construction of the screw round the cylinder (which is performed by a wedge bent round it), and the evolution of the spiral line from the cone, not being generally understood among mechanics, I have been induced to lay before your readers the following simple illustrative diagrams.

Fig. II represents an orthographical cylinder, and the semicircle 1 2 3 4 be its plan or circumference: this cylinder we will suppose is required to be cut into a screw. To do this, the number of revolutions or threads to be cut must first be determined: if there be four, five, or more worms, it will then require less power to turn the screw than if it were less vermiculated: here the screw has only two revolutions. To construct them, first draw the line *a* 8, at right angles to the cylinder, on which line set twice the circumference as shown by the figures 1 2 3 4, 8 1 2 3 4, &c. next erect the line 8, *p*, perpendicular to the base line 8, and place the height of the cylinder thereon: then from *p* produce a line to *a*; this will now represent the surface of the wedge, which wedge is to be represented as furled round the cylinder in the following manner. First, draw the lines 1 2 3 4, &c. perpendicularly up the surface of the cylinder, and on the wedge; next produce parallel lines from the inclined points *h h h*, and perpendicular to 8 *p*, until they [illegible] the cylinder: the points of intersection on the surface will then be the points through which the curve or thread must pass: this is the construction and a development of the screw. For the spiral line, first form a cone, as at fig. L, of the same height and diameter at the base as the cylinder; then set one leg of the compasses at the apex of the cone *t*, extending the other leg to *r*, and with the radius *t*, *r*, describe the curve *r* 8; on this curve set sixteen divisions, four being equal to the circumference of the base of the cone, and the sixteen equal to two revolutions round its base: next converge these lines towards *t*: this being done, take from the wedge of the cylinder the length of the lines 4 *h*, 4 *h*, &c. and set them on the lines 4 *b*, 4 *b*, &c. of the conic evolution: next set the compasses on t, and revolve the lines $b\,b$, &c. to come in contact with the cone: these lines then draw parallel to the base of the cone; and where they intersect the inclined lines on the conic surface will be the undulating or serpentine line; the spiral one will now be seen to a demonstration by the diagram fig. O above, which diagram is formed by the revolutions round the respective points brought up from the cone below Figs. DEFG show the conic sections by a very simple method, which is new*. i, i, for example, shows the cone. Now suppose the line $t\,t$ to be the section-line for the hyperbola, (a section perpendicular to the base of the cone,) take [illegible] distance $t\,u$ at the base, and set it to h, b, on each side of the line h; next set the compasses on the cone at O, and revolve round the dotted lines $c\,c$; then take the measures $t\,r\,r$, and set them on each side the line h: these lines draw perpendicular to $b\,b$, and produce the parallel lines $t\,t$, and then at the intersections of these perpendicular lines with the horizontal ones will be points through which to describe the hyperbola. [illegible] the parabola (a section cut parallel to the side of the cone) [illegible] the distances 1 2 3 at fig. D, and set them up the line 1 [illegible], F; these lines draw parallel to $d\,d$; then from the cone take the measures $e\,c, e\,c$, and set them on each side the [illegible] at fig. F, which lines draw perpendicular to $d\,d$, and the intersections with the parallel lines will give the points through which to describe the parabola.

For the ellipsis (a section cut obliquely through the cone), find the centre of the section-line marked 1, 2, at fig. D, and from the apex of the cone draw a dotted line to the base through 1; then with the centre n revolve round the dotted line to x. The dotted line a is also produced at right angles to the dotted line i, n, proceeding from the apex of the cone; next draw the line 1, m at right angles to the section line 1, 2, and revolve the dotted line $i\,a$ round to m; then will the [illegible] 1, m be the minor, and the line i, 2 the major diameters of the ellipsis; to describe which, take the two diameters and place them at fig. G, then form the figure as there shown: this is plain to inspection.

III. *On Spandrel Groins.*

I have also sent for insertion in your next number, a drawing of the new and more effectual method of building groined arches in brick-work, than the common one generally adopted (termed spandrel groins), with the centring and moulds laid down as used for executing the same. These kinds of groined arches are better adapted for subterraneous structures, and better answer the purpose for large warehouses where immense weights or burthens are required above them, than the common method of vaulting, they being less liable to fracture at the groined angles by any extraordinary impulse operating on the crown of the arch. This system of vaulting, I believe, has not yet been published in any work. As far as I can learn, they were first executed by Mr. Alexander, architect, at the London Docks; and recently by Mr. [illegible], architect, at the new Custom-House.

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* The sections of the Scalene Cone I have shown in my Treatise on the Practical Principles of Perspective.

BB represents the ichnography of the caps of the piers with the abutments of the arches, as shown at KR. Fig. P. represents the scenographic view of the groined vault as executed; *xxx* the elevation of one of the centres made of two-inch deal board rising circularly six feet, which is one-third the span; the whole opening being 18 feet. *r t*, *r t*, &c. represent the angle rib of the same thickness traced from *h* 6, *h* 5, &c.; *m m* shows the plan of the ribs, which are two feet apart; *nn* a square centring box; and *uuu* the ichnography of the boarding on the centring. These boards are each 1½ inch thick: at the London Docks 1¼ inch boards were used, the arches there being one brick and half thick. Fig. R shows the mould by which to cut or form the ends of the boards to the centring. This mould is made in the following manner: Take the hithermost divisions 1 2 3 4 5 6, at *z*, and set them on the line 1 2 3 4 5 6, at fig. R; these lines draw parallel and at right angles to the line 1, 6; next take the divisions at the plan *h n*, *h n*, &c. and transfer them to 6 *p*, 5 *p*, &c. and the intersections will give the mould sought for. To find the mould fig. T, for the spandrel of the groin, first draw the lines *a a*, &c. at right angles to the centre line *a* [illegible] then take the height *h*6, *h*5, &c. and set them on the lines [illegible] *a c*, &c.; by this means you will form the angle curvature or middle rib: next take the separate distances *c c*, &c. and set them on the line *ee*, &c. at fig. T. These lines draw parallel and at right angles to the line *ee*; lastly, take the distances *an*, *an*, &c. and set them from *e* to *o* (the corresponding divisions) seen at fig. T; this will now give the length and bevel of each board round the spandrel, which is all the moulds required in constructing the centring in this way; observe the line *o o*, &c. at fig. T is a straight line: one side of the bricks in the arch (but not visible in this drawing) has a concavity on their surface for a key to the mortar; this is formed by the mould when made.

The brick-work for leveling above the groins is grouted: in some cases groined vaults contain but a single arch; flooring joists are laid over their tops and filled up between with dry rubbish. It is necessary to say, the piers of the former groins are built with Aberdeen and the caps with Dundee granite, it being found the hardest and best calculated for this purpose: observe, the joints of the stones must be left open round the piers until the groined arches are well settled, otherwise the stones at the joints will flush or chip off. This has been found to be the case in practice.

The drawing in outline marked fig. S shows a better and more advisable method of centring these groins, where considerable labour and wood may be saved. For example, P, &c. are piers of the groins, LLL ledgers of deal quartering running horizontally along each bay, and supported on [illegible]; *ccc*, &c. plans of the centres, as ranged and fixed on the ledgers: each bay or passage being centred in this manner, next board them over, forming them into a semicylinder, as a common vault or area. This being done, next get out a mould for marking the common intersections of the cross arches: to do this, take the divisions 0 1 2 3 4, and set them on the line 0 1 2 3 4 at fig. M; these lines draw parallel to each other and at right angles to the perpendicular line 0 4. Next take the distances *a b*, *a b*, &c. and transfer them to 4 *n*, 3 *n*, &c. this will then give the curvature of the mould, and which mould is seen further applied on the ichnography of the centring. Jack ribs, as *r r*, &c. are next to be regularly fixed on the cylinder, and boarded over, making the whole of the centring by this means appear as the centring of a common equal pitch cylindric groin. To form the spandrels of the groins, first take the divisions *t t*, &c. and transfer them to the line *h h*, &c. at fig. N; next take the measures *ed*, *ed*, &c. and transfer them to *h i*, *h i*, &c. at fig. N: this will then give the mould whereby to mark out the spandrels on the centring, and which mould is then applied thereon. Lastly, bridging pieces, as shown at *g g*, &c. are to be fixed across the angles and boarded, which form the spandrels and complete the centring for receiving the brick-work. Thus is the whole sufficiently and practically explained.

XXXVI. *Answer to Remarks on Mr.* Donovan's "*Reflections on the Inadequacy of electrical Hypotheses.*"

To Mr. Tilloch.

Sir,— With regard to the remarks contained in your Magazine for December last, upon my "Reflections on the Inadequacy of electrical Hypotheses," I beg leave to offer a few observations.

Amongst a variety of statements rather unusual in philosophical controversy, it is there mentioned that I observed but half of the necessary phænomena, that it was the wrong half, &c.; that I passed my errors through the Kirwanian Society, and the Royal Irish Academy, and in the latter case so successfully as to obtain the prize from that learned body. It was principally on these accounts that, the author of the remarks undertook to detect my errors, which, as he chooses to assert, had thus received the assent of two literary institutions. Half at least of this task was unnecessary; the paper in question never was read in the Academy, and never obtained the prize. The essay which really did obtain the prize I sent in December 1813, and was on a very different subject; the essay on electricity was read in the Kirwanian Society nearly two years before*. The inaccuracy of the above-mentioned statement is therefore surprising; and the more so, when it is considered with what caution and deference a learned body of men (including many fellows of the university, and most of the literary characters of the country.) should be treated by an unknown individual. This much I felt it my duty to say with regard to the Academy; and but for this, I should have been disinclined to occupy the pages of your Magazine, or obtrude myself on the notice of its readers, concerning a trifling question at issue merely as to facts.

Extensive reading would have shown that the electrical states attributed in my "Reflections" to the Leyden phial had not been noticed by me alone. Experimenters of great reputation had observed analogous facts, and of this a perusal of the works of Wilson, Eéles, of the *Encyclopædia Britannica* (art. *Electricity*), and of various other authorities that I now forget, will afford ample testimony; so that the correctness of my experiments stands supported by the concurrence of persons of high reputation. The question does not relate merely to the states of the phial, but comprises the whole doctrine of *plus* and *minus*: they are inseparably connected; and as the hypothesis stands, one part cannot be maintained without contradicting the other. Why, therefore, should so limited a survey of those objections be taken, which are all in harmony; and why need one part of the hypothesis be sustained when it is opposed by the other?

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* This is distinctly noted in the abstracts of the Kirwanian Society.—Phil. Mag.

In my "Reflections, &c." I gave a caution concerning the experiments there detailed. They are of a delicate kind, and I stated that they only succeed in certain states of the weather. The period when I made them was dry sunny weather, and I have never found them to succeed easily but in the middle of summer. This is a source of fallacy, which from the season could not have been guarded against in the counter-experiments.

There are various other sources of error. This is very difficult to determine what body is positive or what negative; for positive bodies will, under certain circumstances, attract positive bodies; and negative bodies will attract negatives. Of this also no notice has been taken.

The electrometer of Bennet is an instrument not to be depended on without acquaintance with a principle of electricity which I have developed, and which in a work soon to appear will be shown to have misled many able investigators. Any electrometer on the same principle, whether pith-balls or gold-leaf, is liable to the same objection. In the counter-experiments this error has not been guarded against.

To the sarcasm and wit affected in the paper alluded to, I answer but with silence. I conceive them below the dignity of philosophy, and have seldom seen them employed but in the absence of sounder arguments. The attempt evident throughout the whole, of making my humble labours appear nugatory, and myself little better than an idiot, produces no other effect than to compel me to decline further controversy, and to recommend more temperate and cautious investigation of the opinions of others.

I have the honour to be, &c.

Dublin, Feb. 23, 1816. M. DONOVAN.

XXXVII. *Some Meteorological Observations made in the Netherlands. By M.* VAN MONS, *of Brussels**.

We have this last year remarked a singular opposition between the usual meteorological indications and the weather.

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* Communicated by the Author.

The air scarcely ever became dry without the barometer descending instead of rising with the storm, and the lowering instead of the rising of the thermometer most frequently preceded the rain. The rapid risings and fallings of the barometer were followed by weather more or less fine, and it rained at an elevation above the "*very fine weather*," while it was dry with a lowering below the stormy point; and the hygrometer, like the manometer, harmonized with the reversed indications of the other instruments.

Winter opened with a frost of 11° of Reaumur the first day: this extraordinary cold will break the elasticity of the air, and a long thaw with continual rains will be the consequence of it: this thaw is besides indicated by all the habitudes of animals and plants: all the larvæ are found at the surface of the ground, and the roots of plants have risen; no retreat of any insect is deep, and they are rather put out of the reach of the rain than of the frost. Astronomical appearances indicate the same thing, and augur a humid and warm season from beginning to end. Acute rheumatisms (but as yet there is no intermittent fever) prognosticate the approaching humidity of the season. There have been scarcely any but north winds during the whole year, and these have lasted whole weeks at a time. The wild or mountain plants have uniformly overtopped those sown, and the harvests have thereby suffered much. The fruits of autumn are still (February) green, and the fruits of winter, and even of spring, are eatable.

The extraordinary temperatures of the season, either of heat or cold, have not as usual been followed by extraordinary opposite temperatures. The barometer rising with the stationary thermometer has not once indicated fine weather, and in general the motions of the thermometer do not seem to have been influenced by the inverse movements of the barometer. Nevertheless the fine and prolonged rains, which I call the rebounding rains (*pluies de retente*) have most frequently been the precursors of dry weather. The decrements of the moons have almost restored calmness to the air. I saw during the serene weather of September the thermometer ascend and the barometer descend at 11 o'clock at night; and when I rose, both instruments had returned to their station by day. At 3 o'clock no movement was remarked, and during the whole month the barometer was very changeable, although the manometer was stationary, and during the whole year the needle of this instrument, at other times so true, marked falsely. This proves that the air presses more by its elasticity than by its weight; and the winds, contrary to custom, seemed to be the cause instead of the effect of the changes;—I say *seemed to be*, because it is never so in fact.

The briskness of the fires has rarely indicated that the oxygen in the air was owing rather to vapour than to an azotic compound. The influence which propagates frost acts from south to north: this alone accounts for its short duration. It is at the points of passage of a line which from the ruling star stretches to the centre of the earth, that remarkable changes are successively manifested: a similar re-action, at least a regular one, has not been observed this year. The revolutions in the body of the sun, the eruption of several volcanoes in this body, the short duration of its revolution on its axis, &c. which have been perceived at the Milan observatory—could all these have influenced the variations detailed?—Time will show.

B. Van Mons.

XXXVIII. *Sequel to the melancholy Catastrophe at Heaton Colliery.*

> " Gloomy and calm, attendant on the close
> Of all our pangs I sate.
> ——— At length by fate compell'd,
> On the cold pavement one by one expir'd,
> Grovelling among the dead——
> ——— I surviv'd,
> And tardy fate with supplication tir'd "
>
> Boyd's *Translation of* Dante's *Inferno*, canto xxxiii. stanzas 11, 15.

To Mr. Tilloch.

Sir, — In your number for last June, you did me the favour to print the narrative I transmitted of the inundation at Heaton Colliery, by which accident thirty-three men and forty-two boys were either drowned or irrecoverably inclosed in the higher recesses of the mine. The steam-engines having at length drawn out the water, I am enabled to communicate the sequel to this tragedy. On the 6th of January the first body was found: it was that of an old man employed on the waggon-way; and a fact worthy of notice is, that the waste water in which he had been immersed had destroyed the woollen clothes and corroded the iron parts of a knife the deceased had in his pocket; yet his linen and the bone haft of the knife remained entire. Shortly after Mr. Miller, the under viewer, the wasteman, and a few others were discovered: they had met a similar fate, having been overtaken by the water about a hundred yards from the shaft to which they had been hastening to save themselves. But the lot of these eight persons may be considered fortunate; for their sufferings were transient when compared with those which awaited the unhappy beings left at work towards the rise of the mine, and as yet unconscious of their dreadful situation. About the 16th of February the higher part of the workings was explored,—and now a scene truly horrible was presented to view; for here lay the corpses of fifty-six human beings, whom the water had never reached, being situated thirty-five fathoms above its level. They had collected together near the crane, and were found within a space of thirty yards of each other. Their positions and attitudes were various;—several appeared to have fallen forwards from off an inequality, or rather step in the coal on which they had been sitting—others, from their hands being clasped together, seemed to have expired while addressing themselves to the protection of the Deity—two, who were recognised as brothers, had died in the act of taking a last farewell, by grasping each other's hand,—and one poor little boy reposed in his father's arms. Two slight cabins had been hastily constructed by nailing up deal boards, and in one of these melancholy habitations three of the stoutest miners had breathed their last: and what seems singular, one of them had either been stripped of his clothes by his surviving companions, or had thrown off all covering from mental derangement. A large lump of horse flesh wrapped up in a jacket, nearly two pounds of candles, and three others which had died out when half-burned, were found in this apartment, if it can be so called. One man, well known to have possessed a remarkably pacific disposition, had retired to a distance to end his days alone and in quiet; and that this would be the case was predicted by many of his fellow-workmen who were acquainted with his mild temper. Another had been placed to watch the rise and fall of the water, to ascertain which sticks had been placed, and was found dead at his post.

There were two horses in the part of the mine to which the people had retired. One had been slaughtered, its entrails taken out, and hind quarters cut up for use; the other was fastened to a stake, which it had almost gnawed to pieces, as well as a corf or coal basket that had been left within its reach.

An important question must now occur to every friend of humanity For how long a time did these ill-fated people exist in their horrible abode? Unfortunately, to this inquiry no precise answer can be given; but that they perished for want of respirable air, and not from hunger and thirst, is certain; for most of the flesh cut from the horse, together with a considerable quantity of horse beans, was unconsumed, and a spring of good water issued into this part of the colliery: besides, the unburned remains of candles afford evidence of a still stronger nature:—and by these data the coroner's jury was enabled to pronounce a verdict accordingly. The overman had left the chalk-board on which it is usual to take down an account of the work done, together with his pocket book, in an empty corf. On these some memoranda might have been expected to be noted; but no writing subsequent to the catastrophe appeared on either. Two of the men's watches had stopped at four o'clock;—this period of time might be somewhat more than twenty-four hours after descending into the mine: but it is also probable they may have wound up their watches after the accident had taken place; and notwithstanding various reports, I do not believe that any document was discovered to throw light on this lamentable part of the subject.

On referring to my former letter, it may be seen that the owners of Heaton mine opened the shaft of an ancient colliery situated about 300 yards from the place where the pitmen were known to be at work; but, owing to innumerable falls from the roof, and the prevalence of carbonic acid gas and carburetted hydrogen gas, were prevented from penetrating further than 80 yards into the waste. On a Wednesday morning the accident happened, and by the following Saturday the scaffold which closed the old pit was reached and removed. By these means it is thought by some professional men that the pure air, already much reduced by respiration and combustion, would be let out through the broken coal, and that this would be the utmost possible period of these miserable sufferers' existence. Though it must frequently have fallen to the lot of miners to have been entombed alive in the prosecution of their hazardous avocations, yet I know of but two of these occurrences upon record. The first was published by Dr. Percival in the Memoirs of the Philosophical Society at Manchester for 1785. A pitman, whose name was Travas, had the misfortune to be shut up in a mine at Ashby-under-Line, owing to a quantity of earth bursting into the shaft, which was 90 yards deep. Here he remained inclosed in a cavity three yards in length by two in breadth—in a seam only two feet thick, without either water or food, and where the candles of the workmen who dug him out would not burn—for the space of seven days and nights,—and though perfectly sensible when found, he died in a few hours after.

The other occurrence took place at Beaujoc, in the vicinity of Liege, in 1812, and was detailed in a pamphlet by Baron De Micoud. It nearly resembled the Heaton inundation, except that the water rushed from an upper seam of coal, and not from a waste situated to the rise. This colliery is 186 yards in depth, and its seam 35½ inches thick:—here seventy men and boys were shut up without food or wholesome water, and where their candles could not be kept burning, for the duration of five days and nights; but were at length fortunately extricated by a drift being driven from an adjoining mine. In this case I am led to think the quantity of deleterious gas had been less considerable than at Heaton.

I have now only to remark, that the bodies of those men which had laid in wet places were much decayed; but where the floor was dry, though their flesh had become shrivelled, they were all easily recognised by their features being entire.

Yours,

Newcastle-upon-Tyne, March 4, 1816. N.

P. S.—Eleven more corpses remain undiscovered in the recesses of the mine.

XXXIX. *Some Account of certain Agates presenting by an artificial Arrangement the Aspect of organized Bodies.* By *M.* Gillet de Laument, *Engineer to the Mines of France*[*].

M. Moreau de St. Mery, having brought from Italy some agates found in the bed of the Trebia, which discharges itself into the Po near Plaisance, sent a polished one to M. de Montegre, who showed it to me at the Institute on the 9th of Oct. 1815.

I immediately observed that the appearance of the organized body which this agate presented seemed to have been given it by art. We showed it afterwards to several naturalists, who at the first glance thought they saw in it the marks of the wood of the palm-tree; while others thought they saw the traces of a marine body. In fact, this agate presents in the middle some round conical bodies penetrating into the stone, the summits of which are at the surface, and the united bases of which form an appearance of a network with hexahedral meshes: in other parts of the stone we find only small insulated cones with circular bases.

Having for a long time remarked the fractures which the blows of a hammer produced on hard and homogeneous stones, I had ascertained that under the place where the blow was given cones were formed, the summit of which was at the point of contact, and of which the base was sunk more or less regularly into the stone; and I had thus formed with fine freestone the *lustrous freestone* of Haüy. According to this observation, I think I may venture to say, that the appearances of organized bodies in the agate in question had been formed by blows adroitly given and struck in succession beside each other.

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[*] Communicated by the Author.

I even made the attempt myself, and I thus obtained agates furnished with cones presenting the aspect of organized bodies.

The agate from Italy seems to have been polished after the blow, which has removed the summits of several cones from the middle of the stone, and has given them a strange appearance. In this agate and in mine, small circles are seen with the microscope at the places where the blows have been given; on wetting, both the cones partly disappear on account of the liquid penetrating the fissures, but on drying all these cones re-appear. The Marquis de Dree has in his fine collection an agate set in a ring, which he showed me lately, and which has cones that must have had the same origin.

The object of this observation is to inform amateurs that foreign dealers know how to produce in certain agates very pretty effects, by an artificial arrangement, which they frequently give out to be natural, having effected it so as completely to deceive the eye.

Account of an Aërolite which fell in Moravia, and a Mass of Native Iron which fell in Bohemia. By the same.

Chevalier de Schreibers, superintendant of the Imperial collection in natural history at Vienna, has communicated an account of an aërolite exhibiting striking anomalies from all those hitherto known. I presented to the Geological Society of Paris two pieces of this variety: the smallest was given me at Milan, by Father Pini in 1818, as having fallen in Moravia; but not being affected by the magnetic needle, and containing no iron in a native state, like all the other stones which have fallen from the atmosphere, I doubted its reality as an aërolite: several persons even regarded it as a piece of a crucible. The largest piece was given me by M. Schreibers: it is still covered with its crust almost all over; it is not affected by the magnet, contains no iron in a metallic state and no nickel, and is lighter than the common aërolites of the same volume. It presents a black shining and as if shagreen surface, which distinguishes it from other aerolites at the first glance. M. Schreibers personally ascertained that this stone really fell in Moravia, at Slavnera near Iglaw, on the 22d of May 1808.

The same gentleman gave me another very interesting piece: it is entirely native iron, and detached from a mass weighing upwards of 190 pounds, which fell at Ellenbogen in Bohemia. This piece has since been cut into the shape of a coin. It has the peculiar property when placed in weak nitric acid of being attacked unequally, and of then exhibiting blackish particles, and also some whitish in relief, which have a curious arrangement with respect to each other, and which seem to flow from a law of crystallization which one would think led to the octahedral form. I think that the blackish parts are iron surcharged with carbon (steel), and the white parts iron. It seems probable that the blackish portions being first consolidated have as it were crystallized into the mass of iron when still liquid or soft. M. Schreibers regards this arrangement as peculiar to all the native iron which has fallen from the atmosphere.

I attempted to treat in a similar way a piece of native iron which fell in Siberia, and described by Pallas: and in fact, small black and white specks became very visible; but in this specimen the iron having been subjected to a high temperature which rendered it cellular, the black and white marks have followed the contours of the cavities, and are very remarkable.

XL. *Answer of* H. *to* G. S. *on the Metallic Salts.*

To Mr. Tilloch.

Sir, — In your Magazine of January last appeared another paper from your correspondent G. S., who, though he apologizes for a small mistake committed in his first, has now strayed so far from propriety, that it will, perhaps, cost him more trouble to extricate himself, than it has to attain that state into which his last has hurried him.

You will observe a passage (page 35) with which he begins: "In support of H.'s assertion, that metallic salts are super-salts with excess of oxide—" This assertion he presses on me: but the evidence of some of your former numbers, in which my papers are inserted, cannot fail to convince your readers, that if this is also a misconstruction of your correspondent's, it must equally display his ignorance of the science of chemistry, and of the substance of my former papers; or should it not be as I have represented, finding that his arguments were in some measure weakened by my last, he wishes to introduce this new assertion on *my* part, so that it may accord more agreeably with his wishes, and enable him to bear down with redoubled violence on my tortured opinion; thus making a poor endeavour to take by this stratagem that which he could not force by truth.

Now do I consequently lay open for him to commence an attack on that opinion which *I* set forth, against which he may probably have as much to say, as against that which he has already framed for me.

With this I conclude, that as it is impossible for the most able chemist to cope with one who is regardless of that on which he pretends to argue, I feel myself completely justified in relinquishing the present controversy: it would indeed be litigious in the extreme for me to continue to oppose one who writes not against me, but against the new system (that metallic salts are super-salts with excess of oxide) of which your correspondent G. S. is the author.

Yours obediently,

London, March 6, 1816. H.

XLI. *On Fulminating Gold.* *By* J. B. Van Mons, *of Brussels*[*].

There is a preparation of gold which presents much interest, and on which the nephew of the illustrious Driessen has published a very good essay:—I mean fulminating gold. This compound may be procured by all the methods of putting ammonia in reaction on dissolved gold; by aqua regia, with which we precipitate gold by the help of a soluble oxide, and by the muriate of gold which we decompose by means of ammonia; by the oxide of gold which we treat with ammonia, either gaseous or liquid or with any ammoniacal salt; and finally, by this same oxide, which we must keep some time in a dark and damp place where the air is stagnant. I prepare it with most advantage by precipitating by potash a diluted solution of muriate of gold and muriate of ammonia. The precipitate is at first muriate of ammonia and gold; but the instant the ammonia becomes free, it is aurate of ammonia, called ammonium of gold, or fulminating gold, which is produced.

Fulminating gold is not decomposed by any acid, and it is not by the alkalies: nevertheless the first of these bodies disunite it by the adjunction of a hyperoxygenated muriate and liquid chlorine; without this adjunction, but with the assistance of heat, double muriate being produced. This happens from the combustibles having more energy than its metal. This compound is a salt in which the oxide of gold performs the functions of acid, and the ammonia the function of an oxide: it is therefore, as I have already said, an aurate of ammonia, which exists by an engagement the more intimate as the oxygen of the gold, with which the oxygen of the ammonia is proportioned, presents more caloric to displace. The acids, in order to decompose this salt, ought either to take the ammonia from it or dissolve at once its two elements; but for the first effect it is necessary that the oxide of gold should be insulated from the oxygen wanting caloric, and for the second effect it would be necessary that the acids should be able to dissolve oxide of this same quality, both which are absolutely impossibilities: and if such effects could be obtained, they would multiply *ad infinitum* the number of oxides, salts, and all other bodies having oxygen in combination.

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[*] Communicated by the Author.

Nevertheless fire and time, by adding caloric to the oxygen of the gold, determine the disengagement of the principles of the fulminating gold and their being taken in solution: energetic combustibles take from the gold the oxygen, with its defect of caloric, reduce [illegible], and set the ammonia at liberty: which explains how, in a globe of fire strongly heated, the fulminating gold is decomposed without fulminating; which certainly would not happen if the ball were platina, gold, or silver.

Ammonia has too little affinity with water to enable that liquid aided by heat to take it from the gold: nevertheless the heat in this case brings together the active principles of the compound; and this happens when we wash it with very hot water: the caloric displaces in the first place the ammonia from the oxide, the hydrogen from the azote, and the oxygen from the gold, and pressure disunites them by forming water. Fulminating gold does not detonate under water, for want of power to take the temperature requisite for this effect; and at a heat a little higher it resolves into its nearest principles, with the single exception of the case in which it has been washed too hot.

Carbon organized and hydrogenated in the oils with ether and alcohol, as well as azote hydrogenated in ammonia, take up the oxide of gold from its solvents; which proves that this body acts more willingly by capacity than by intensity; and the oils may even decompose the fulminating gold by taking up on one hand its oxide and on the other hand its ammonia. The oxide of gold which is separated from its solutions by the contact of the air—will it be aurate of organized azote, or oxide rendered insoluble by hyperoxidation? In this last case, as these solutions are always with an excess of acid, the heating in those of the muriatic acid must determine the resumption in solution of the oxide under the formation of oxygenated muriatic acid.

The sulphuric acid highly concentrated decomposes fulminating gold by provoking the union between the principles of the water in order to appropriate to itself this water. The detonating compounds, which the same acid decomposes with an explosion, set the oxygen at liberty.

The fulminating gold is formed spontaneously in the same circumstances and by the same causes as the native nitrate of lime: it is the organized azote of the air hydrogenating itself into an oxidule which produces the first body, and the same azote sub-hydrogenating itself into an acid which produces the second; and it may be presumed that the two actions take place simultaneously, or depend upon each other.

Gold in very minute division is dissolved in chlorine by the help of heat, and also when nitro-muriate of gold is partly decomposed by heat, then treated with muriatic acid, and evaporated to dryness. It is a brown substance, very deliquescent, which easily decomposes the water of the atmosphere, forming muriate of gold. I have not as yet examined it with precision.

On Silver. [By the same.]

It will not easily be believed, as Brugnatelli observes, that silver, which is a metal reducible *per se*, may be oxidized in the air, and at a heat which its oxide scarcely requires in order to be reduced. There must, therefore, be some other cause than oxidation which renders silver vitrifiable: and this cause will deserve examination. A very intense heat, or the electric fluid,—do they organize a portion of the oxygen of the primitive matter which forms the basis of the silver, in such a way as to originate an oxide different from the ordinary oxide of that metal? This is a difficult question. Such an effect, by displacing hydrogen with the oxygen of the primitive matter, will compose a metal more hydrogenated, and which will be irreducible in the fire; and the oxidation will take place not by the oxygen of the air, but by that which is proper to the primitive matter.

We must inquire if the oxide of silver obtained by fire possesses other qualities than the oxide of the same metal obtained by the acids; and in the contrary case, we must ascertain if the reagents employed in this examination do not separate the supercombined hydrogen, proportioning at the same time the oxygen of the caloric to the degree of the new hydrogenation; or if, by oxidating the hydrogen of the oxide at the ordinary degree, they do not separate some of it from the water—two circumstances which will bring silver back to its first constitution.

We know already that the metals are oxidated by the electric fluid, as well in vacuo as in contact with the air. The experiments relative to the oxidation of silver by caloric alone must be undertaken on a very large scale in close vessels, on account of the volatility of the metal, and in vacuo: we must particularly ascertain if the metal, in returning to its primitive state, undergoes a loss of weight.

If the fact of the mineral organization of the primitive matter of a metal by the electric fluid, or by a red-heat, is confirmed; we may explain how in aërolites the metals may be artificially composed, and the nature as well as the mode of organization of the primitive matter of the globe will thereby be elucidated; and we shall extract oxygen from a substance in which its presence is least expected. This fact will, besides, denote that the metals the oxides of which are reducible by fire, owe this quality to a smaller dose of oxygen, which is at the same time the cause of their greater weight.

Juncker had already ascertained that at a very intense heat silver is converted into a fused oxide; and Richter mentions the fact of a considerable quantity of silver which a modern alchemist had kept in the fire several years, being thereby transformed into a mass of fused oxide. M. Richter having obtained this mass, tried to reduce it, first by heat and afterwards by the usual reducers, but he was able to extract only a very small portion of metal.

Silver, on being organized, may also become an acidifiable combustible; for between an oxide and such a body the distance is not great: and if azote is hydrogenated into ammonia, and if tellurium and arsenic, by an inverse action, from metals become acidifiable combustibles, silver may very well, on receiving caloric, without losing hydrogen, become a double super-combination of this principle, and constitute either an acidifiable combustible or the oxide of a different metal, more energetic, because it has more hydrogen, and because with precautions we may obtain it reduced. This will be the second artificial metal, and others will soon follow, which, like ammonia, will be decomposable, because it will exist by composition, and in which caloric, favoured by a disposing affinity, may be substituted for the hydrogen of the new metallization, and allow the oxide of silver to be reduced, *per se*, into its metal: but hydrogen at a lesser heat might also be substituted for the water of the new metal, and thus present it to us reduced. How can we see oxide of silver resist reduction in the fire, without making such a phænomenon the subject of the most serious meditation?

XLII. *On the Cosmogony of Moses, in Answer to Dr.* PRICHARD. *By* F. E——S.

To Mr. Tilloch.

SIR,—I FEEL no inclination to involve Doctor Prichard in controversy; but as he has had the advantage of fully stating the grounds of his opinions, given in your Magazine, relative to the Mosaic cosmogony, it will, I presume, be allowed me briefly to examine whether he has succeeded in removing the objections against them indicated in my former letter.

In the paper* which occasioned my observations, Doctor Prichard, after having, as he imagined, exhibited a complete coincidence between the series of facts detailed in the first chapter of Genesis and those inferred from geological phænomena, remarks, that "if this coincidence is surprising in itself, it appears the more so when we compare the cosmogony of the Hebrews with the notions on this subject that prevailed among other nations of antiquity. *We find invariably that all other speculations on this subject are founded on some fanciful analogy* with natural processes that are daily observed." In his recent communication†, however, our attention is particularly directed to, and our surprise excited by, "the remarkable connexion discovered between the primitive histories of the most remote nations on the earth and these documents embodied in Genesis." It is added that, not only the Asiatic nations, but the Runic scalds of Iceland and Scandinavia, and the ancient priests of Mexico *were equally in possession of the primitive traditions;*" and the latter certainly never obtained them from Jerusalem‡. Other similar facts are adduced in support of the opinion that the early parts of Genesis are a compilation: but in maintaining it, Doctor Prichard does not "place the author of the Pentateuch in the rank of common compilers of historical fragments possessed merely of natural intelligence;" nor does he regard the supposed materials of the compilation, "in their origin as common historical testimonies." On this hypothesis I shall merely obseve, that if in relating events which no uninspired person could ever *with certainty know*, the author of the Pentateuch had recourse to records and traditions of whatever character or antiquity, it could only be to supply the want of immediate inspiration. It seems, therefore, that to preserve consistency, either the hypothesis or the inspiration of Moses must be abandoned.

I now proceed to the points in discussion. In my former observations the word *day* is admitted to have been figuratively employed in Hebrew, as it has, I believe, in every language, to signify an indefinite portion of time; but I contended that in *the six days creation* it cannot be so understood; because, for no other discoverable purpose than to guard the term against a metaphorical interpretation, its meaning is there expressly confined to the duration of an evening and a morning, to the decay and the return of light, the limits of a natural day. This limitation, reiterated six times in terms as explicit as language can supply, causes no difficulty whatever to Doctor Prichard; "for," he remarks, "if we use the word *day* to signify a portion of time, and *have occasion to allude to the beginning or end of the period designated*, we always carry on the metaphor and adopt the corresponding terms." The truth of this position is obvious; but its application, instead of supporting the tropical sense contended for, affords an additional objection against it. In the day and night of Brama, the duration of the respective portions, the preceding and following twilight, the day, and the night, are all chronological periods; and a distinct notion is conveyed in properly sustained figurative language of the relative time of the exertion of Brama, when it is said, "At the close of his night, having long reposed, he wakes, and awaking exerts intellect." Had the morning and the evening been thus used in Genesis, to mark precedence or subsequence of different acts of creation in the same day; had it been said, The sun was made in the morning, the moon in the evening of the fourth day, the language would with equal propriety have borne a literal or figurative interpretation. But it is remarkable, that neither the morning nor the evening is once alluded to for any such purpose: the acts of each day are enumerated, and the day itself is said to consist of an evening and a morning. This being so, if *day* be understood figuratively, there does not appear to have been the slightest "occasion to allude to the beginning or end of the period designated" by it; since no other information would be conveyed by the supposed allusions, than that an indefinite period was composed of a metaphorical evening and morning; or, in other words, that one indefinite period consisted of two indefinite periods. I do not, however, pretend to appreciate the value of this information, estimated "according to the genius of Hebrew literature."

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* Phil. Mag. No. 210, p. 265. † Ibid. No. 214. ‡ p. 112.

The other, and more weighty objection,—that it does not appear that the Hebrew people ever understood the six days of the creation as equivalent to so many indefinite periods,—seems implicitly admitted by the remark, that had Moses read a lecture on geology to "the shepherds of Goshen, and told them what space of time each oceanic deposit occupied, and by what organic remains it is to be recognised, he would have spent his time to little purpose." Moses, however, told "the shepherds of Goshen" many things quite as difficult to comprehend as that each epoch of the creation had occupied an extensive period: nor does it seem at all easier to conceive creation the work of six days than of as many thousand years. With respect to successive oceanic deposits, each characterized by peculiar organic remains, no trace of such a series of events is discoverable in the Mosaic account. On the contrary, in that narrative, the waters retire previous to the existence of animated beings, and never again cover the earth until the days of Noah; and the work of creation is represented as simply progressive, which is little consonant with a series of creations and extinctions of whole races antecedent to the formation of man. Could these apparently irreconcilable discordancies be overlooked, still Doctor Prichard's parallel between the facts detailed in Genesis and those inferred from geological observation would fail. According to the sense in which the twentieth and two following verses of the first chapter of Genesis have hitherto been generally understood, all the inhabitants of the w[illegible] were called into existence on the fifth day; while geolo[illegible]ence places the orders of testacea and zoophytes in the thir[illegible] that is, in a period when, according to Moses, nothing animate was created. It is true, Doctor Prichard thinks their deficiency in loco-motion sufficient to exclude them from the fifth day's creation. But besides that this criticism may be suspected of over-refinement, it is to be observed that, if they be excluded from that day, it is absolutely certain that Moses has assigned them no other. When, therefore, zoophytes for their analogy to vegetables, and testacea without participating in that analogy, are removed from the fifth to the third day, and we are afterwards called upon to admire the coincidence of the series of the facts detailed in Genesis with those inferred from geological phænomena, Doctor Prichard seems to forget that the coincidence is effected by his placing two orders of animated beings where Moses never placed them.

I am, sir,

Your very obedient servant,

Bath, March 10, 1816. F. E——s.

P. S.—The conjecture thrown out at the close of my former letter has no higher pretension than a simple possibility. In making it, I was perfectly aware of the objection which Doctor P., observes may be drawn from physical considerations set forth (not in the "*Système de la Nature,*" but) in Laplace's *Exposition du Système du Monde.*

XLIII. *Observations on the late excessive cold Weather.* By WM. SKRIMSHIRE, *Esq. of Wisbech.*

To Mr. Tilloch.

SIR, — IN consequence of the public solicitation of your valuable correspondent Mr. T. Forster, as well as for the information of the other readers of your useful Magazine, I am induced to send the following observations respecting the late intense cold as experienced in this part of the kingdom.

My thermometer is graduated according to Fahrenheit's scale, and I have no reason to doubt its accuracy. It is placed out of doors facing the NE, with a projecting edge of brick-work to defend it from the rays of the rising sun.

My barometer is a wheel instrument, therefore I do not rely on it for extreme accuracy, but as affording sufficient information of the vicissitudes in the pressure of the atmosphere. In my observations with this instrument, the letters *r. f. s.* denote that the mercury is either rising, falling, or stationary, at the time of noting the observation.

I generally register my observations in a morning only, but on particular occasions they are repeated oftener, especially between nine and ten at night. The journal is not kept, perhaps, with that accuracy and regularity which are necessary to meet the public eye, but merely for my own amusement, and as connected with observations on the culture of certain plants which at present occupies my attention.

February 1816.

Tuesday 6th.—Eight o'clock A.M.

Wind NE. brisk.—Barometer 28·95 *f.* Thermometer 36°.

Sky dull, with small rain and sleet.

Nine o'clock P.M. The sleet this morning was soon succeeded by snow, of which we had three or four showers during the day; but this evening it fell faster, and now continues on the ground. Barometer fallen to 28·80 *s.*

Wednesday 7th.—Nine o'clock A M.

Wind NE. Barometer 28·80 *s.* Thermometer 30°.

Much snow has fallen during the night; and still continues to fall. It lies on the ground about three or four inches deep, on a level.

Ten o'clock P.M.—It cleared up soon after the morning observation was registered; but we had several slight showers of snow during the day.

The barometer began to rise this evening. The sky is now clear, and it is a very sharp frost, the thermometer 20°.

Thursday 8th.—Half past eight o'clock A.M.

Wind N.—Barometer 29·07 *r.* Thermometer 26°.

Alternate snow and sunshine. It has been a severe frost in the night, and still continues. My well-water pump is frozen for the first time this year. This pump is situated in a warm Rumford kitchen, which is in use daily from eight o'clock in the morning till nine at night. I make this remark because it satisfactorily proves to my mind, that the thermometer had been several degrees below 20° during the night.

Ten o'clock P.M.—The atmosphere was dull this afternoon; but clearing up in the evening, a bright moon-light night succeeded,

ceeded, with an intense frost; the thermometer being now at 10°.

Friday 9th.—Half past eight o'clock A.M.

Wind S.—Barometer 29·30. Thermometer at zero!

The sun bright. A brilliant sky. Not a cloud to be seen in the hemisphere. The air feeling intensely cold.

Ten o'clock P.M.—The frost has continued very severe the whole day.

The thermometer rose to 10° [illegible], and is now at the same degree.

Saturday 10th.—Nine o'clock A.M.

Wind SE. Barometer 29·34 r. Thermometer 10°. Atmosphere rather dull.

Ten o'clock P.M.—A little snow fell this evening. The thermometer rose to 22° during the day, and is now at 26°.

The weather continued with the common cold of winter till Thursday the 15th of the same month, when it began to thaw, and the barometer, which had been stationary at 29·91 for three days, began to fall in the evening. The next day was very hazy, and the thaw continued with a falling barometer, and the thermometer at 42°.

I leave it for others who have more leisure and abilities than myself, to settle the apparent discrepancy in the accounts that have been published respecting the intense cold with which this kingdom has lately been visited. But I must confess that one of the motives which has induced me to give this public testimony of the facts that I noticed, was an observation in the Meteorological Report, published in the Monthly Magazine for February, where the reporter boldly asserts, that he is "*quite sure that those who talk of the thermometer being at* 6° *or* 26° *below the freezing point are under a mistake.*"

Now, sir, I do not pretend to be either wiser or more accurate than others who have paid attention to this subject, but am "*quite sure*" that I was wide awake when I examined my thermometer;—aye, and at the time too when I noted in my journal the facts above related. And having been in the habit of making meteorological observations for nearly 30 years, it is very probable that I may have acquired some portion of exactness, if not of facility in reading off figures from the graduated scale of a philosophical instrument, at least within *half a dozen degrees!* And I do aver, that at half past eight o'clock on the morning of Friday the 9th ult., Fahrenheit's thermometer stood at zero! and as the sun rose with splendour at a quarter past seven o'clock on that morning, I have but little hesitation in believing that at two hours before sun-rise the thermometer was some degrees below zero!

Recollecting the degrees of cold which are destructive to different vegetables growing in this kingdom, that even turneps on the ground in winter are injured by 24° and totally destroyed by 30° below the freezing point; we may naturally conclude that great devastation must have been committed by the late severe weather: but the destruction would have been unparalleled in this kingdom, had not the ground been previously covered by snow, especially as the sun rose with splendour on the morning of the 9th ult., thus greatly augmenting the danger to the living principle. Therefore, when we reflect upon these circumstances, what a powerful source of gratitude they ought to prove in our minds, to that great and beneficent Being, who has spread such a protecting mantle over the fertile fields of this highly cultivated country!

I remain, sir,
Yours, &c.
Wisbech, March 8, 1816. **Wm. Skrimshire.**

XLIV. *On Aërostation.* *By* Richard Lovell Edgeworth, *Esq. F.R.S. &c. &c. &c.*

To Mr. Tilloch.

Sir, — There is something apparently selfish and ungracious in claiming inventions that are published by others. But in philosophy, as well as in law, a fatal error is often committed by *laches*,—or, in plain English, by leaving unclaimed, for a length of time, a right that belongs to us.

The ingenious Sir George Cayley has frequently proposed to impel flying bodies, by letting them descend obliquely through the air, and forcing them in a contrary obliquity against the air by impelling them upwards. He refers to Mr. Evans's paper in your Magazine for November last. Now, sir, I did actually apply this invention to a small fire-balloon at my own house in the year 1786, which confirmed me in the opinion of the practicability of this invention. But as there were a great number of thatched houses in my neighbourhood, I desisted from making further trials.

In 1802 I became acquainted with M. Montgolfier at Paris, when two of my friends who were with me, asked M. Montgolfier whether any practicable means of directing balloons had ever been communicated to him. He replied, "Never, but in a letter from a gentleman in Ireland." One of my friends immediately inquired, whether that letter had been communicated to him in the year 1782, by the Marquis de la Poype, of Bourg en

Bresse. He said that it was. "Then," said my friend, "that letter was written by Mr. Edgeworth, for I saw it when it was dispatched."

Montgolfier was extremely pleased; took the trouble of explaining to me many of his ingenious contrivances, particularly his *belier hydraulique*.

I cannot omit to mention an instance of M. Montgolfier's candour and generosity. After having taken considerable pains to obviate objections which had been made to his parachute, I communicated to him the means of rendering the descent of the parachute perfectly safe and equitable.

When we were going away, we passed through the magnificent staircase of the *Conservatoire des Arts et Metiers*, Rue St. Martin. "There," said he, pointing to the dome, "is an instance of national gratitude; you see the remains of my original balloon suspended from the roof, and my parachute annexed to it. I will have my parachute taken down, and yours substituted in its place."

These, however, are relations resting merely on assertion,—but in the Transactions of the Royal Irish Academy, vol. vi. page 101, there is in an Essay of mine the following note·

"A balloon may be carried forward with certainty and celerity in any direction where there is no perceptible wind, if it is alternately lowered and elevated by altering from time to time its specific gravity, which may be done by various means, without losing much hydrogen gas; and if it be furnished with fins or small sails, and be set to a proper angle, with its line of ascent and descent, their vertical pressure against the air will impel the balloon forward."

" Swift manœuvres his Laputa in this manner:

" I tried this invention on a small balloon in the house of the late ingenious Dr. Usher, (Professor of Astronomy in the University of Dublin), the friend of science, and of those who wished to improve it."

I do not mean to insinuate in the most remote manner, that Sir George Cayley or Mr. Evans knew any thing of my invention; probably they never heard my name; and far from wishing to derogate from their merit, I shall with pleasure contribute to a subscription for carrying on any practicable plan for the advancement of aërostation. Earnestly hoping that it may be applied to better purposes than those to which it has been hitherto unworthily confined,

I am, sir,
Your obedient servant,
RICHD LOVELL EDGEWORTH.

Edgeworthstown, Ireland,
March 6, 1816.

P. S.—One of the most powerful means of retarding the progress of aërostation has been ridicule. In the Essay to which I have alluded there is the following passage:

"The disposition to ridicule every scientific project as absurd, until it has been absolutely brought to perfection, has been the common topic of complaint amongst men of inventive genius; and it is curious to observe, that poets, who suffer so much themselves by the taunts of men of the world, and by the [illegible]hy of the vulgar, should in their turn revenge themselves upon men of science, and treat their speculations with disdain. Ben Jonson has attempted this, in one of his masques, with a degree of humour which is not always the portion of those who throw ridicule on science. Merefool, the clown of the piece, consults an adept, who promises to instruct him in all occult secrets, and to show him apparitions of all the learned men of the ancients; but every man who is called for happens to be busy, from Pythagoras, 'who has rashly run himself upon an employment of keeping asses from a field of beans,' to Archimedes, who is meditating the invention of

'A rare mouse-trap with owls' wings,
And a cat's foot to catch the mice alone.'

"Not only the same taste for ridicule, but the same ideas, we find repeated, with a slight alteration, at different æras. Aristophanes and Lucian amongst the ancients; and Butler, Swift, and Voltaire, the three modern masters of ridicule, have in various shapes the same ideas, and are alike disposed to confound the ingenious and the extravagant. The best way of parrying the stroke of ridicule is to receive it with good humour; Laugh with those who laugh, and persevere with those who labour, should be the motto of men who possess the power of invention.

"The late Dr. Johnson, who in his Rasselas ridiculed this idea of the art of flying, lived long enough to see the ascent of the first air-balloon."

XLV. *Report made by Messrs.* HEURTIER, PERCIER, *and* DUFOURNY, *to the Class of Fine Arts of the French Institute, of the Labours of the French Architects at Rome during the Years* 1812 *and* 1813*.

GENTLEMEN,—You will recollect that the labours prescribed by your regulations to the pupils who are architectural pensioners of the French government at the Academy of Rome are divided into two classes.

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* *Magasin Encyclopédique*, Nov. 1815.

The one comprehends *studies of the details* which during the first three years of their residence they ought to furnish, in order to show how they had been employing their time, and the progress which they are making in the study of the art; works the property of which remains with the author.

The other description of labours consists in the restoration of ancient monuments, which the pupils ought to undertake during their residence, and present in a finished state at the expiration of their term. These restorations, which we may regard as the completion of their education, appertain to government.

Your commission has followed the order of this division, in the examination which you have directed them to make of the works sent from Rome this year. Their report will embrace, first, the analysis of the studies of annual details; and secondly, that of the restoration of antique monuments.

M. Suys, one of your pupils, has extended his researches over the three orders of architecture.

In order to study the Doric and Ionic orders, he has judiciously chosen those of the theatre of Marcellus, as the most correct of the edifices of ancient Rome, and even of Italy. Of the three drawings which he has made, one traces the whole of the two orders raised on each other, with the arcades made in the intercolumniations: the two other drawings, the details of each of these orders; *i. e.* its base, capital, and entablature.

As to the Corinthian order—among the excellent models contained in ancient Rome, M. Suys gives the preference to that of the temple of Mars the Avenger, which, better than any other perhaps, demonstrates how, by adding boldness to grace, the genius of the ancients knew how to impress character on front elevations the most elegant and rich.

Of the four drawings which he has given of this monument, the first gives the plan and general elevation, taken from the work of Labacco, and which he has borrowed. The other three drawings present the elevation of two of the three intercolumniations which are still standing, and the working details of the base, capital, architrave, and rich ceiling which covers the portico.

M. Suys deserves no less praise for the care which he has taken to point out throughout the nature of the materials, and their fine execution, than for the skill with which he has given the character of the precious ornaments with which this order is enriched. By following this course M. Suys may expect success at every step of his career.

It is certainly surprising that Desgodetz, so careful in general in collecting the monuments of ancient Rome, should have totally neglected the Trajan Pillar, one of the most perfect [illegible] Rome. Perhaps he regarded it as a monument of sculpture rather than of architecture; perhaps also, ignorant as he was of drawing, he despaired of being able to give its beauties.

However this may be, M. Chatillon has given a proof of his taste in selecting this splendid monument of antiquity for one of the subjects of his studies.

He has executed five drawings; which present, 1st, the plan and elevation of the whole column, and surmounted, as it was when entire, by the statue of the emperor Trajan: 2dly, The façade of the pedestal at the entrance side, with the base and capital of the column: finally, two fragments of another face of a pedestal, which exhibit the trophies, eagles, and other ornaments. These studies or figures are exceedingly well drawn, and are calculated to give a favourable idea of those which M. Chatillon has since made on the portico of Octavius, and the transmission of which is announced.

M. Provost has directed his attention to the remains of the Temple of Jupiter Tonans.

We know that, buried almost entirely under the ruins of the edifice of the Mons Capitolinus, on the slope of which it was constructed, this temple has long exhibited no other remains than three isolated columns, forming the right angle of the peristyle of its anterior façade: they were besides buried in such a way as to show only the upper part of their shafts, the capitals, and part of the entablature: so that none of the architects who since the revival of the art have published collections of antiquities, had either seen or given the *toute ensemble*.

M. Provost, profiting by the excavations made around the feet of those columns under the auspices of the French government, during his stay at Rome, not only surveyed them entire, but executed four drawings of their details.

The first contains the total elevation of the intercolumniation which forms the angle of the interior façade, from the level of the ancient soil to the summit of the entablature, with all the various plans, sections and profiles, necessary to its development.

The three other drawings are consecrated to the details; such as the base and the capital, the entablature of the sofite of the architrave, and the frieze of sacrificial instruments which runs along the flank of the temple.

Thus (thanks to M. Provost) we now enjoy for the first time a complete view of the Temple of Jupiter Tonans, one of the most magnificent of ancient Rome; and that without contradiction which unites in the highest degree the luxury of ornament with delicacy and neatness of execution.

The most essential particulars in this new Corinthian model [illegible]:

1. The difference of breadth which exists between the two intercolumniations of the angle; that of the anterior face having three modules, whereas that of the flank has only two and a half. This kind of irregularity has been occasioned, according to all appearance, by the great proximity of the ancient edifice called Tabularium, which, not admitting of giving the flank of the temple the accustomed depth, constrained the architect to reduce the breadth of the intercolumniations.

2. The height of the column, which, being carried to ten diameters and a fourth, presents one of the most slender and elegant Corinthian proportions with which we are acquainted.

3. The nature of the stylobate, which consists of a simple marble die, without base or cornice, and one diameter high, placed under each column.

4. Finally, the steps which, practised in the intervals between the dies, served for the ascent from the ancient way to that of the peristyle of the temple. This practice of placing the steps in the thick part of the pedestals of the columns had not been before remarked, except in the ancient Temple of Minerva at Assise. The present is a new instance, which deserves to be imitated on all occasions, when it is not permitted to bring in front of the peristyle the steps by which we are to ascend it.

M. Gauthier has presented various interesting studies for the years 1812 and 1813. They consist in a drawing of the console of the arch of Titus; in the restoration of the Temple of Jupiter Stator; and in that of the Temple of Peace: the whole forming fifteen large drawings.

The console placed at the key-stone of the arch of Titus, one of the finest which we see in ancient monuments, is very beautiful; besides the foliage and other ornaments with which it is enriched, it presents in its anterior front a figure standing, very much decayed, which, according to the attributes still perceptible, seems to have represented the City of Rome, with the helmet on her head, supported on a lance, and holding a crown of laurel. M. Gauthier has drawn this console both in front and in profile, in a broad and soft manner, and in the proportion of half of the real size; a task which has not hitherto been attempted.

In three other drawings he gives the plan and elevation as restored, of the Temple of Jupiter Stator. For a long time, as is well known, the remains of this temple were reduced to three isolated columns, and of which a portion of the shaft, the base, and the stylobate were buried so deep, that hitherto those parts had not hitherto been observed. M. Gauthier, profiting by an excavation in 1813, by the orders of the French government, around those columns, and carried to the level of the first cou[illegible] of their foundations, ascertained that they had, not steps, (as indicated by Palladio,) but one continued stylobate formed of large blocks of stone covered with marble slabs; and consequently these columns must have formed part of the lateral peristyle of the temple.

These observations, and the precise measurements which he has taken to [illegible] those parts, have afforded him the [illegible] of fixing with certainty the general proportions of the whole, left incomplete to the present time; and consequently to restore in a suitable manner the plan of the temple and the entire elevation of its façade. The capital and the entablature are given with great truth, and in such a way as to give a just idea of the magnificence of this monument; one of the purest models which students can possess for beauty of general proportion, gracefulness of profile, and the exquisite taste of the ornaments with which they are enriched without profusion.

The Temple of Peace, which M. Gauthier made the object of his studies in 1813, did not present in his opinion the same kind of merit. Certainly the same purity is visible both in the general character of this monument, and in the distribution, taste, and execution of its ornaments: we cannot even refuse to admit that the *ensemble* of its character resembles strongly that of the Thermæ and other edifices built in the time of Dioclesian, the first æra of the decline of the art: but the magnificent arrangement of the plan, the colossal dimensions of the masses, and the means of construction employed to ensure the necessary stability, furnish a [illegible] source of investigation.

For these reasons the Temple of Peace has from the earliest times fixed the attention of studious architects. From the first æra of the restoration of the art, Serlio and Palladio published the drawings of it; an example which was followed by many others, particularly by Desgodetz, who, more exact, corrected the mistakes of his predecessors, but cannot give as yet any thing but an incomplete work; because, in his time as at present, the remains of this edifice were buried under enormous ruins, which rendered every inquiry fruitless.

What artists have long desired, the French government has undertaken. By excavations executed by their orders in 1812 and 1813, the entire surface of the ground-floor of the building was cleared, and which in some places was covered with more than twenty-five feet of earth.

M. Gauthier followed step by step these excavations, and measured and drew not only all the vestiges of the plan of the edifice, but even the smallest fragments of ornaments which appeared to be adapted to throw light on its decoration: and the result of these enquiries has been given to the world by him in twelve drawings; three of which exhibit the plan and elevation of the edifice in its present state of ruin, with the indication of the parts uncovered in the excavations: and in the other drawings, the monument is restored entirely, with all the details of the orders which decorate it both externally and internally.

The most remarkable parts of this restoration, because they are absolutely new, are:

1. The plan, section, and external elevation of the portico which ranged along the whole eastern part of the edifice, and preceded its entrance on the side of the Colyseum: the compartments of the pavement in cipolline and yellow antique marble, and those of the groined arches adorned with caissons.

2. The plan, more exact and complete than hitherto drawn, of the three naves composing the body of the temple, with all the compartments of its magnificent pavement formed of the most valuable marbles, such as, violet *breche*, the yellow antique, the green granite, porphyry, and serpentine.

3. The entire decoration of the grand circular niche which occupies the middle of the north flank, restored according to the fragments of bases, columns, capitals, and entablatures discovered in this part. The bad style of these fragments and their coarse execution, added to the vestiges of a Christian altar found also in this place, lead the author to conjecture that the decoration of this niche has undergone changes at the time of the edifice being converted into a church.

4. The lateral entrance made in the flank of the temple opposite to this niche, and which ought to have looked into the *Via Sacra*.

5. The measurements in detail of the grand column now raised in the plan of St. Mary Major, and which, as we already know, was taken from the Temple of Peace. It results from these measurements, given for the first time with precision, that this column presents the singularity of being thickened about one-third up the shaft, and diminishing very little at its upper part: without doubt, in order to present more strength to the arch, and more resistance to the burthen which it had to support.

6. Finally, the most valuable of these restorations, because it was the most unexpected, is that of the decoration of the arch which covered the central nave of the edifice. It has been drawn from the numerous fragments discovered in the excavations, collected and combined by M. Gauthier, so as to reproduce not only the arrangement of the various square caissons, lozenges, ovals and triangulars, of which it is composed, but the minutest ornament in stucco with which it was enriched; so

that there remains nothing more to desire upon this most important part of the decoration of the edifice.

It appears that the excavations hitherto executed have [illegible] presented any indication which proved that this edifice had been connected with any other; whence it has been hastily concluded that it has never formed part of a palace or a suite of baths, as generally thought. Nevertheless, before giving up this idea, it would be necessary to try an excavation which has hitherto been neglected. This is of the ground comprehended between the Temple of Peace and the two halls adjoining, vulgarly called the Temples of the Sun and Moon. This excavation, well directed, would perhaps discover the ancient communications which have been suspected to exist between these edifices with the mere foundation; as the Temples of the Sun and Moon exhibit a striking analogy to the Temple of Peace, both in their construction and in the compartments in stucco with which their arches are adorned.

Such a discovery would remove every doubt: for the more attentively we consider the edifice in question, in whole or in parts,—the style of its architecture being evidently inferior to that which prevailed in the time of Claudius and Vespasian, to whom the erection has been ascribed, and particularly the arrangement of its plan, which, similar to that of the halls of the baths or palaces, has nothing in common with the well-known arrangements of the temples of the ancients,—the more we are persuaded that tradition is defective respecting the true destination of this edifice.

As to its date, it is sufficient to cast our eyes on the drawings of M. Gauthier, who has with the most praiseworthy fidelity given the true character of the details, in order to be convinced that ornaments of a style and execution semi-barbarous cannot be the work of the Augustan age of architecture.

M. Gauthier has added to his drawings an explanatory notice, in which he gives not only the description of the various parts of the edifice, but also all the details of its construction, which he has studied with the greatest care, besides every information relative to the excavations, in which have been found the materials which served as his authority for establishing with certainty the restoration of the received parts of this vast monument.

In a word, the restoration of the Temple of Peace, which was not obligatory on M. Gauthier, does honour in every respect to his zeal and talents, and cannot but augur well for that of the Temple of Mars the Avenger, which he has just finished for the government. His work is the more precious, as, several fragments which served as his authority being already destroyed, it is not possible now to make one more complete. This last con-

sideration

sideration ought to induce the class to ask M. Gauthier, to whom [illegible] are indebted for this restoration of the Temple of Peace, to present the same to government, in order to be added to those which it already possesses, and among which it will hold a most distinguished place.

The bare review of the works presented by Messrs. Suys, Chatillon, Provost, and Gauthier, for their annual tribute of 1812 and 1813, demonstrates that, so far from having neglected to fulfil the obligations imposed by the regulations, they have even overstepped them. Their zeal in this respect does not merit less eulogy than their eagerness to profit by the excavations, ordered with so much munificence by the French government, of most of the monuments of Roman magnificence.

While on this subject, it may be necessary to remind our readers that from the year 1809 to the end of 1813 the French authorities at Rome have incessantly excavated and laid bare, for the sake of the advancement of erudition and the instruction of architects, the remains of the chief edifices of ancient Rome: it is thus that we have successively seen exposed to view the temples of Antoninus and Faustina, of Vesta, of Fortuna Virilis, of Concord, of Jupiter Tonans, of Peace, of the Sun, the Moon, and Jupiter Stator; the arches of Janus, and of the Goldsmiths, the column of Phocas, Trajan's Forum, the baths of Titus; and lastly, the Colyseum.

The restorations of ancient monuments, made for the government by Messrs. Leclerc and Huyot, and of which the com[illegible] is about to give an account, will not be less interesting.

Among the ancient edifices which time has respected, none is more imposing, better preserved, or rather less damaged, than the Pantheon: none is better calculated to give an elevated idea of the genius of the ancients for architecture; for even at the [illegible] it was erected (which was the most flourishing æra of the art) it was regarded as one of the most beautiful *chefs-d'œuvre*. It is in this way that Pliny, Dion Cassius, Suetonius, Ammianus Marcellinus, Spartianus, &c. speak of it.

The moderns have paid the same adoration to it: from the happy epoch of the revival of the [illegible], learned men and artists have unceasingly made it the subject of their meditations and studies. The most celebrated architects, Serlio, Palladio, Pietro Ligorio, Fontana, Desgodetz, Piranesi, have published the measurements; and the two latter in particular with such precision that their works appear to leave nothing to be desired.

This subject had not therefore the advantage of novelty; nay, it was even to all appearance exhausted. M. Leclerc, however, has chosen it as the subject of one of his *restorations*, and he has no reason to complain of want [illegible]

In a series of twenty-one drawings executed with the [illegible] rare intelligence and on a very large scale, M. Leclerc has [illegible] only revived as it were, his subject by the immense developments which he has given both to the *tout ensemble* and to the details, and by the art with which he has developed them; but he has succeeded in giving a more complete work than any one has done hitherto, and new even in many respects in consequence of the numerous discoveries with which he has enriched it.

In the state of impossibility in which the committee find themselves of indicating all of them, they will content themselves with describing the most important, beginning with those which are relative to the external parts of the temple

1. Hitherto we have not had an exact drawing of the flight of steps by which, from the place in front of the Pantheon, its portico was attained. This external part of the edifice having been long covered by the successions of rubbish thrown on the adjacent ground, each had drawn it according to his own ideas, his taste, or fancy. M. Leclerc, according to the observations made and the vestiges discovered at the time of the excavations of 1804, in front as well as upon the flanks of the portico and of the body of the temple, has restored this interesting part in its true form.

He shows that the ground of the surrounding square was paved with large irregular flags of Travertine marble, and that the temple was ascended by means of four steps of white marble; at the top of which was a broad pavement or foot-path which extended not only over the whole of the façade, but even over the outskirts of the circumference of the temple, where it was sustained by a continued *stylobatum.*

2. Vitruvius in the second chapter of his third book, treating of the various thicknesses to be given to columns according to the different breadths of the intercolumniations, prescribes the thickening of a fiftieth part of the diameter of the columns placed in the angle of the porticos; "because," he says, "the great light which strikes them more than the others making them appear more slender, it is necessary to remedy this false appearance, which would otherwise deceive the eye."

M. Leclerc has verified that the column of the angle of the portico towards the right of the spectator is in fact stronger than the others by a fiftieth nearly of its diameter; the column of the opposite angle does not exhibit the same conformity with the precept of Vitruvius, but we know that it was placed there at the time of a very modern restoration.

3. The two great niches which occupy the bottom of the aisles of the portico, stripped for a long time of every indication of covering, [illegible] and without any

kind of ornament by Serlio, Palladio and Desgodetz, a barenness which agrees badly with the richness of the general appearance. Piranesi is the only person who of late years has suspected that those niches were formerly covered with marble. M. Leclerc, pushing his observations further, has ascertained by the arrangement of the holes of the rivetings perceptible in the bricks, that not only were they covered with marble throughout their whole circumference, but also that they had an impost, which received a fillet or archivolt of marble also which stretched around the ceinture of the niche.

4. He is also the first who remarked, above the interior cornices of the portico, the chases cut for receiving the bronze arches which cover the three naves of the portico: an observation which, added to the authority of Serlio, Pietro Ligorio and Palladio, has afforded him the means of restoring in its primitive form that part of the portico the magnificence of which exceeded all idea.

5. The three cornices, which begird at different heights the external circumference of the temple, had not been drawn exactly either by Desgodetz or Piranesi. M. Leclerc has ascertained the true profile, as well as the stucco ornaments of which he has discovered the remains: so that we may regard as new the details which he gives of those three cornices.

6. He has also examined with care the upper part of the creeping cornice of the front, in order to ascertain if there had been at the extremities pedestals for supporting statues, as Serlio, Palladio, Desgodetz and Piranesi have drawn them; and he has ascertained that there were pedestals on the pediment only, and not at the angles.

7. This inquiry, for which he was compelled to remove the tiles which cover this part, led him to perceive a range of holes extending above the whole of the creeping cornice of the front: a certain proof that the front was surmounted by a frieze of ornaments cut in marble or bronze, and which he has consequently thought himself entitled to give in his drawing.

8. Not content with having developed, in a particular elevation and section, the ornaments and all the details of construction of this frontispiece, from the first course of the foundations to the summit of the pediment, M. Leclerc formed the happy idea of bringing together, on the same paper and on the same scale, the details of the construction of other antique frontispieces which time has respected. Such are the Doric frontispieces of the small Temple of Pæstum and that of Hercules at Cori, the Ionic frontispiece of the Temples of Fortuna Virilis, and of Concord at Rome, and the Corinthian frontispiece of the portico of Octavius at Rome. In short, this [illegible] as it is instructive,

structive, which, exhibiting under one and the same point of view the parallel of the construction of the best preserved ancient frontispieces, facilitates the comparison of the materials employed, as well as that of their section, their ornaments, and other means of execution.

9. Doubts had been always entertained respecting the simultaneous construction of the two frontispieces which we remarked at the façade of the Pantheon. Those of good taste, and even the most enlightened artists, shocked at this vicious double employment of the frontispiece, which being only a mere representation of the roof ought to be essentially unique in a building, did not hesitate to pronounce that these two fronts could not have existed together in the primitive state of the Pantheon, and that doubtless the posterior frontispiece formed the summit of a first façade, on which had been placed afterwards the portico and its frontispiece.

However plausible this opinion may be, it must give way to the evidence of the result obtained by M. Leclerc: according to a series of observations too long to detail, but directed with the greatest sagacity, he has concluded, and demonstrated by his drawings, not only that the horizontal cornice of the posterior frontispiece has not been cut with a view to introduce therein the frontispiece of the portico, but that it has been always thus from the origin of the edifice, and that the intimate connexion of the construction of these two frontispieces with that of the body of the edifice does not admit of a doubt that they had been constructed at the same time.

10. One of the most important points to elucidate, because it serves to decide what has always been the true destination of the monument, was to examine if, as a number of writers have asserted, the Pantheon originally formed part of the baths of Agrippa, of which it must have formed one of the principal apartments. By an attentive examination of the posterior part of the edifice, to which are still attached considerable remains of those baths, M. Leclerc has ascertained that in fact they were immediately contiguous to the Pantheon, that the masonry of the two edifices was intimately connected to a certain height, that even the second exterior cornice of the Pantheon went round and seemed to have prevailed on the body of the edifice for the baths; but that there was no aperture and no orifice to warrant the idea that there had ever existed between them any interior and direct communication: hence he concluded that from its origin the Pantheon had always been a temple.

If from the exterior of the Pantheon we pass with M. Leclerc into the interior, we shall find that his studies and inquiries have not [illegible]

11. Some writers have asserted that the small buildings or tabernacles, which to the number of eight are distributed around its circumference, were added at the time of the consecration of the temple to the worship of the Christian religion: this mistake is entirely cleared up by the developments which M. Leclerc gives of their construction. These demonstrate that it is and always has been connected with that of the body of the edifice; and that those altars or tabernacles have existed from the origin, and have undergone no other alterations than those occasioned by successive reparations and the change of religion.

12. The friezes of running palm-leaves, which M. Leclerc has drawn on the frontispieces of these altars, as well as on the cornice of the interior order, are fully warranted by the holes which he found in these parts, indications which had not previously been observed.

13. Finally, various sections taken on the great semicircular and square niches distributed around the interior circumference, show that the construction of the Corinthian order which decorates them, is so connected with that of the body of the temple that it becomes impossible to suppose, as has generally been done, that this order was added afterwards.

Such are the observations and researches which led M. Leclerc to conclude:

1. That the building of the Pantheon, from its original construction, has been destined to serve as a temple; and never formed part of the baths of Agrippa, with which it has not any direct communication.

2. That the Corinthian order which decorates the interior, and that of the entrance portico, date from the epoch of the primitive construction of the edifice, and are not additions made to the circular part of the temple subsequent to the time of Agrippa.

If we refer to those who have written on the subject, we find all of them to be of a contrary opinion. According to Michael Angelo, quoted by Vasari (Life of Sansovino), the Pantheon was the work of three architects, the first of whom built the interior order to its entablature, the second the attic, and the third the external portico.

In order to set aside an opinion so generally adopted, other methods than simple reasonings are wanted. M. Leclerc was aware of this; and freeing himself from all spirit of system, guided merely by the light of actual observation, he has had the merit of being the first to throw [illegible] light on those important questions, by means of almost [illegible] demonstration.

The results which he thus [illegible] deserve the more confidence, that they have not been [illegible] by [illegible] after having been discussed with the most enlightened antiquaries and artists of Rome, to whom he communicated them on the very ground as soon as the discoveries were made, and who were obliged to sacrifice their opinions to the evidence of their senses.

These results are besides perfectly in unison both with the inscription which the frontispiece bears, and with Pliny, the only one of the ancient writers who saw this monument entire; for by using the words *Pantheum ab Agrippa factum*, he has evidently testified that the erection of this building as well as its completion was owing to the munificence of the same man: the conception and execution were consequently the fruits of one and the same genius.

To this rapid sketch of the principal observations of M. Leclerc the committee ought to add: 1st, That he has particularly endeavoured to give their true character to the ornaments, and to represent with the most scrupulous exactitude the nature, the arrangement, even the colour of the materials; in short, all the details of construction, unnoticed for the most part or badly indicated hitherto. 2dly, That he has added to his drawings very satisfactory explanatory notes, containing the description of the monument, the indication of the sources where he got it, and the authorities to which he refers for restoring the lost parts. 3rdly, That the numerous drawings which M. Leclerc has dedicated to this fine restoration are executed in every respect with a perfection to entitle them to serve as models of their kind; and that henceforth we cannot expect to see the Pantheon, that *chef-d'œuvre* of ancient grandeur, grace, and harmony, represented in all its details with more intelligence, purity, and precision.

M. Huyot has presented the restorations of the celebrated Temple of Fortune, as well as the Forum of the ancient city of Præneste.

We know that this city (now Palæstrina) is situated in Latium, twenty-three miles from Rome, towards the east, and rises into an amphitheatre on the slope of a mountain forming the extremity of one of the branches of the Apennines.

Its origin is of the highest antiquity. Cicero says that he cannot assign the epoch of it: what appears certain is, that long before the founding of Rome this city had its gods and temples, the most considerable of which was that of Fortune, who was afterwards worshipped throughout all Italy.

When Sylla assumed the dictatorship (B. C. 82) Præneste contained buildings of great importance. Cicero speaks of its walls as being of an *opus* [illegible], or irregular polygons, which he says was a construction [illegible] among the Dorians: he speaks also of a large space exposed to the sun, which he calls *Solarium*.

According to Suetonius, the Forum occupied the space contained between the public road and the foot of the temple: it had two ground plans, the one inferior and the other superior, and its porticos were adorned with statues. There were also to be seen there, according to Varro, two Basilicæ, one called Æmilia, the other Fulvia, and a Gnomon.

Livy speaks of a statue of Jupiter Imperator, who was held in great veneration, and who doubtless had a temple in the Forum. Authors also mention temples of Juno, Mars, the goddess Opis, and particularly the famous temple of Fortune, which situated at the top of the city commanded it, and filled it almost entirely with its appendages which extended to the Forum.

After having had kings and various kinds of governments, Præneste subjugated by the Romans under the conduct of Cincinnatus, destroyed then restored by Sylla, who sent a colony there, flourished during the emperors, and preserved its splendour to the fall of the empire.

From this period Præneste declined, and in the various devastations which it suffered its monuments were destroyed; on the ruins of which the modern Palæstrina has gradually been raised.

The latter increasing from time to time now presents the singular spectacle of houses, churches, and high palaces, suspended as it were on the vast terraces which supported the ancient edifices.

It has been through those modern buildings that M. Huyot has set about seeking out and measuring the remains of the monuments which formed the splendour of the ancient Præneste.

The enterprise was the more difficult, as he embarked upon it almost without guides: those only whom he could consult were Pietro Ligorio, among whose manuscript drawings a general plan and perspective view of this temple with its atrium and appendages were found; Pietro da Cortona, from whose drawings a plan and general elevation were engraved on a large scale; and Suares, who in his *Præneste Antiqua*, and Volpi in the *Vetus Latium*, have inserted perspective views.

Inexact and incomplete as these works are, and the fruits of the imagination rather than of the observations of their authors, they could offer but very feeble assistance.

Piranesi, that indefatigable investigator of ancient monuments, has published nothing on the latter; there was therefore a new mine to explore, and M. Huyot acquitted himself with the greatest success.

He has given six drawings of [illegible]ults of his extensive inquiries. The first three give the plan, elevation, and general section of the ancient Præneste, in their present state, and stripped of the modern constructions which cover them; the three other drawings present the plan, elevation, and general sections of the ancient city, with its chief edifices, restored in the state in which they must have existed at the time of Sylla, an epoch selected by the author to make the restoration.

We there ascertain the ancient Prænestine way, which leads from Rome to the principal entrance of the city; its walls and various gates; the easy flight of steps which lead to a first platform, on which was established the Forum of the city; which in its lower part comprehended the Basilicæ of Æmilia and Fulvia, the Temples of Juno and Æsculapius, and the pools of water for the use of those who came to visit the Temple of Fortune; and in its upper part the Temple of Jupiter Imperator with its porticos.

Above the Forum are several terraces and flights of steps of easy ascent, constructed of irregular polygons, which led to a second platform containing the atrium and the dependences of the Temple of Fortune. Lastly, this Temple itself erected on a piece of ground still more elevated and crowning the whole.

In order to appreciate correctly the merit of such a work, we must consider the obstacles of every kind which it was necessary to surmount.

The immense extent of these edifices, and their situation on the abrupt slope of a rugged mountain, were already sufficient to render very difficult the operations necessary for drawing the plans of their ruins: and how much must the difficulties have been increased, by the complete superposition as it were of a modern city, the streets, squares and public buildings of which intersect in every direction and disturb the arrangement of the ancient city!

If we reflect that it was in spite of these obstacles, that it was necessary first to draw and establish in an accurate manner the respective directions and levels of this multitude of terraces made at different altitudes on the brow of the mountain; afterwards to ascertain the remains of the ancient monuments scattered over the platforms which separate these terraces; to discriminate between them and the modern construction with which they are often confounded by the identity of the materials; finally, to unite and arrange all these fragments, and to form a whole out of them, which should agree at once with the notices left by authors, the vestiges still existing, and the rules of art; we may easily suppose that, to succeed in such an enterprise, it would be necessary to join to a consummate practice in the study of antiquities a [illegible] of the rarest description.

To leave nothing unaccomplished, M. Huyot has added to his drawings a very copious memoir, and which he hopes to have the honour to read to the class, in which collecting all the notices scattered through writers, he gives a sketch of the history of Præneste, and treats of the ancient names of that city, its origin, of the various spots of ground which it occupied, and of its successive extensions; of the monuments with which it is embellished, and of the date of their construction: lastly, he describes the modern city of Palæstrina, and the remains of the Temple of Fortune, the special object of his study.

This last part is the most interesting for the art, because it contains the proper notions for determining the style and character of the architecture of the monuments of ancient Præneste. Although the most ancient, perhaps, of those which we know in Italy, they are those only in which we remark the simultaneous employment of the three orders of architecture.

In short, according to the fragments found in the lower part of the city, we find that the Doric order was employed in the lower part of the Forum: its fluted column with sharp ridges and without base, and the profile of its capital, resemble closely those of the Temple of Hercules at Cori, and the portico which is known by the name of *Pabularium*, the remains of which are seen on the Capitol. A stylobatum still preserved in its place, in the upper part of the Forum, exhibits also the Doric character, the frieze of its entablature being adorned with triglyphs and wreaths, executed precisely in the style of those which we see on a sarcophagus of the Scipios.

The Ionic order entered into the decoration of the edifices dependent on the Temple of Fortune: its capitals, if we are to judge from those which have been found, were very badly sculptured, and nearly of the same kind with those of the ancient portico which is still seen at Perousis.

As to the Corinthian order, it is that which we meet with most frequently in the buildings at Præneste: although repeated ten times, it is remarkable that its proportion and ornaments are always the same: its chapter is decorated with acanthus leaves turned up, and friezed like those of the capital of the Temple of the Sybils at Tivoli, and its height is a single diameter only, conformably to the doctrine of Vitruvius. The cornices of this order are profiled in a very simple manner, and exhibit no kind of ornament.

With respect to the construction, all the walls, either of the terraces or of the body of the buildings, are executed in the *opus incertum*, composed sometimes of immense blocks of stone cut in irregular polygons, and placed on each other without cement; sometimes of thin stones [illegible] with lime:

in several parts of these edifices we see arcades with full ceintures, the arches of which exhibit sections regularly furnished.

These various observations, when compared, lead the author to conclude that, the monuments of Præneste having both by the manner of their construction and the style of their architecture an evident analogy with the Temples of Hercules at Cori, and of the Sybils at Tivoli, with the *tabularium* of the Capitol and some other remains of antiquity scattered through Etruria, their erection dates from an epoch at least contemporary, *i. e.* far anterior to the foundation of the empire, and that consequently the style of their architecture may give an idea of that which was brought by the Greek colonies who came to Italy.

This hasty sketch of a work which has cost the author three years labour, during which he made ten voyages to Palæstrina and a stay of six consecutive months, will be sufficient to demonstrate its importance.

In extricating from its ruins the ancient Præneste, M. Huyot has not only made us acquainted with one of the most celebrated monuments of antiquity, of which but a very imperfect idea had been entertained, and one of the vastest and most magnificent architectural conceptions of the genius of the ancients; but also a style of architecture little known hitherto, and which for the sake of the art ought to be more noticed.

In all these respects the work of M. Huyot is worthy of the approbation of the class, and he has to thank the government for permitting him to continue a year longer at Rome.

Such is the result of the examination which your committee has made of the works sent lately by the French architects studying at Rome: this anaylsis, succinct as it is, will be sufficient to give a high idea of their importance: and if, looking back ten years, you recollect the various restorations already made, and the architectural works published or engraved since their return to France, you must acknowledge that the progress of French architecture has been increasing. Among the works published during these ten years by the pupils of the School of Architecture, we may distinguish the Ruins of Pæstum, by M. Lagardette; a Collection of Town and Country Houses, by M. Dubut; Tuscan Architecture, by Messrs. Grandjean and Famin; a Collection of the finest Tombs of Italy, by M. Grandjean; the Ruins of Pompeii, by M. Mazois; the complete Works of Vignole, by Messrs. Le Bas and Debret.

There now remain to be [illegible] the *restorations* of the Temple of Patrician Chastity, by M. Dubut; the Temple of Vesta at Rome, by M. Coussin; the Mausoleum of Cecilia Metella, [illegible] the Temple of Antoninus and Faustina,

stina, by M. Menager; the Arch of Titus, by M. Guenepin; the Temple of Fortune at Præneste, by M. Huyot; and of the Pantheon, by M. Leclerc; to which we may also add the restorations of the Portico of Octavius, by M. Chatillon, and the Temple of Mars the Avenger, by M. Gauthier.

It only remains, therefore, for us to solicit the minister of the interior to supply the funds for engraving these splendid remains of ancient architecture, to enable us to outstrip the career of other nations.

XLVII. *On a new Method of obtaining pure Silver; with Observations on the Defects of former Processes.* By M. Donovan, *Esq.**

There have been four principal methods proposed for obtaining pure silver. The first is by cuppellation; the second, by the reduction of *luna cornea*; the third, by precipitating silver from its nitrous solution by sulphate of iron; the fourth, by means of that symmetrical precipitation called *arbor Dianæ*.

These different modes labour under defects. They are either inconvenient, uncertain, tedious, uneconomical, or inadequate.

In the process of cuppellation, it is true that all the base metals are carried down by the lead; but it is not thus possible to separate the gold: this is accordingly found in the resulting silver.

The reduction of *luna cornea* by alkali affords when the process succeeds a very pure silver: but the success is doubtful, and for the most part fails in the hands of the inexperienced. It is well known that when the crucible is taken from the fire, instead of containing pure silver, it is often found entirely empty, so great is the tendency of the fused matter to pass through the pores, unless prevented by due skill in the operator. Another source of uncertainty is the degree of heat: if it be too high, the muriate volatilizes: if too low, the reduction is not completed; and without singular good fortune, the expected quantity will in no case be obtained. Beside these difficulties, the process is exceedingly complex and troublesome.

In the next method, a solution of nitrate of silver is mixed with very dilute solution of sulphate of iron: the iron acquires a new dose of oxygen derived from the silver; the latter is therefore precipitated. Unless the sulphate of iron be recently prepared, it will not have the strong attraction to oxygen necessary; and either little silver will be obtained, or none.

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* [illegible], 1819.

But admitting that there is a newly prepared sulphate [illegible] when its solution is mixed with the nitrate of silver, [illegible] tional dose of oxygen is at once presented to the iron. [illegible] the precipitated silver is collected on the filter, there will be [illegible] in it a quantity of sub-oxysulphate of iron, which, if [illegible] taken, can be made by its specific levity to occupy the stratum above the silver.

Another defect in this process is, that the silver is not entirely recovered. If the solution of oxysulphate of iron, from which the silver has been filtered, be mixed with solution of salt, there will be a copious precipitation of muriate of silver. This happens [illegible] when a large quantity of sulphate of iron has been employed; and in these cases, an additional quantity seems to have no effect, with such force do the last portions of silver retain their oxygen. Thus the defects of this process are, that it presupposes the recent preparation of sulphate of iron; that the silver is not entirely recovered; and that what is recovered is not quite pure.

The formation of the *arbor Dianæ* is obviously under the objection of tediousness and trouble.

The defects of these processes compelled me to try several methods for obtaining pure silver. The following appears to answer expectation:

240 grains of standard silver are to be dissolved in as much pure nitric acid of S. G. about 1·2, as will be barely necessary for the solution. This is to be filtered, and distilled water allowed to run through the filter until the fluids amount to two ounces measure. A bright plate of copper weighing more than 64 grains is to be immersed, and frequently agitated in it. When the silver has entirely precipitated, which will very soon happen, the clear supernatant liquor is to be poured off, and the precipitate is to be well washed. The silver is then to be boiled for a few minutes in liquid ammonia: it is to be again well washed and dried on a filter.

A few remarks to show the adequacy of this process may be necessary.

The silver of commerce is composed of 37 of fine silver to 3 of copper. The fine silver is obtained by cuppellation, and therefore contains gold. When this silver is dissolved, the gold is found in the form of a powder. I have sometimes [illegible] it black and sometimes purple; that is, it is either peroxide or protoxide. In the nitrous solution we have [illegible] silver and copper, unless we regard the [illegible] quantity of gold which nitrous acid [illegible]ve.

When into the solution of standard [illegible] plate of copper; the latter [illegible] in place [illegible] which is therefore precipitated. But the copper of commerce always contains other metals: hence we must examine if any of them inquinate the product. Lead, antimony and arsenic have been found in copper. The first and last dissolve in the nitrous acid. Antimony scarcely dissolves in dilute nitric acid, but I find that when alloyed with a large portion of copper it dissolves with sufficient facility. Hence none of the above metals will be found in the reduced silver. When copper is dissolved in strong nitric acid, the solution when saturated lets fall a brown powder which Fourcroy considered oxide of copper. I collected a small quantity of this powder, heated it to a bright red, and found it magnetic. Its saturated solution also struck a black with tincture of galls. This powder is therefore iron, and probably a sub-oxynitrate. But when copper is precipitating the silver from its nitrous solution, the iron, as I have found, will remain dissolved, provided that no heat be used, and that the acid used in forming the nitrate did not exceed s. G. 1·2.

The only metal, therefore, which adulterates the precipitated silver is copper, proceeding no doubt, as Vauquelin observes, from the galvanic action between the precipitant and precipitated metals. It might be doubted that ammonia will dissolve the copper, as it is commonly supposed that this metal in its metallic state is very little, or not at all, acted on by that alkali. The observation, however, does not apply to copper minutely divided. I boiled ammonia on copper powder precipitated from its solution by iron, and well cleansed by washing with dilute muriatic acid: the ammonia in a minute or two assumed a deep blue colour. The silver resulting from the above described process does not afford the least trace of copper to the strictest scrutiny. If the solution of 240 grains of silver be made to amount to two ounces measure, the silver precipitates speedily when the copper is immersed; and the particles are large enough to subside, and to permit the pouring off of the washings without loss.

The quantity of copper necessary to precipitate 100 grains of pure silver is 28·7 grains, according to a great number of trials which all agreed. Bergman makes it 31: but in this case the difference is of no consequence.

The solution from which the silver has been separated affords no precipitate with muriate of soda: hence the separation is complete: and I find that, with a little care, the loss of silver by filtration and decantation need not exceed 3 per cent.

The boiling with ammonia merely takes up the copper: when ammonia is boiled on silver, even in powder, and afterwards saturated with muriatic acid, the transparency of the fluid is scarcely [illegible].

I have been thus minute in my account of so simple a process, because I conceived that new methods are entitled to no consideration unless they are shown to be *adequate* to their end, and *necessary*, on account of some imperfections in the means previously employed. The advantages to be derived from the process now proposed are the recovery of the whole of the silver, the purity of it, and the little time and trouble required for the operation.

XLVII. *On Aërostation.* *By* A CORRESPONDENT.

To Mr. Tilloch.

SIR, — THE enthusiasm excited by the first invention of balloons, has like all the other movements of enthusiasm produced a reaction, which has hitherto involved in a kind of ridicule every attempt to apply them to any useful purpose, and which has been fatal to any progress in their improvement. The steady march of the human mind, however, ultimately triumphs over these alternate fits of extravagance and torpor; and the day is probably not very distant, when the theories of ingenious men controlled by the sober experiments of science will lead to more happy results.

The weight of the machinery necessary to give a mechanical impulse to balloons, appears to be an insurmountable obstacle to that mode of conducting them; and the inclined plane lately recommended by your ingenious correspondents is liable to the same objection, and perhaps in a still greater degree.

Methods must therefore be devised of rendering the atmosphere subservient to their motion and guidance as well as to their support. The contrary currents of air that generally take place at different elevations are well known; and it is presumed that if the balloon were retained near the upper confine of one stratum, and if a small auxiliary balloon were attached by a rope and permitted to ascend into another stratum, the velocity of the former would be sufficiently retarded to admit of its being steered in a course inclined by several degrees to the direction of that current in which it floated: much in the same manner that the skilful seaman *drops* his vessel down a strong *tideway*, by giving her a slight velocity in the *opposite direction*, which enables him to *sheer* and *steer* her at pleasure.

Or, on the contrary, the auxiliary might be made to *descend* in search of a proper resisting current of air.

If the stratum should be too extensive to push the auxiliary beyond its limits, even then the diminished rapidity of the wind at a great elevation would probably produce a sufficient effect.

In crossing the sea, perhaps, a small weight might be allowed to drag through the water, which would effectually answer the purpose of steering the balloon obliquely across the wind, by Sir George Cayley's sail-rudder.

I have the honour to be, sir,
Your most obedient servant and wellwisher,
London, March 17, 1816. T. H.

XLVII. *Notices respecting New Books.*

Mr. Donovan has in the press An Essay on the Origin, Progress, and present State of Galvanism, with a Statement of a new Theory. It is divided into three parts. The first contains a sketch of the history of Galvanism divided into four periods. The second contains investigations speculative and experimental of the principal hypotheses; viz. of those of Volta, Fabroni, and of the British philosophers: of the hypothesis of electro-chemical affinity as maintained by Davy and Berzelius; and of the identity of the agent in Galvanic and electric phænomena. The third part comprises a statement of a new theory of Galvanism, and is divided into two chapters. The first includes the general principles of what has been called the excitement of Galvanism. The second chapter is devoted to the application of these principles in explaining Galvanic phænomena, and is subdivided into five sections. The first section treats of Galvanic decompositions in general. The second presents a new theory of metallic arborizations. The third explains the source of the light and heat manifested in certain experiments. The fourth explains the source of the electrical appearances: and the fifth shows the cause of the muscular contractions and shock.

In this theory the agency of an electric or Galvanic fluid is not admitted; the phænomena are conceived to be explicable by the mere operation of chemical affinity.

Dr. C. H. Parry, of Bath, has just published An Inquiry into the Nature, Cause, and Varieties of the Arterial Pulse; and Dr. G. E. Male, of Birmingham, has given to the world An Epitome of Juridical and Forensic Medicine.

XLVIII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

Feb. 29. A paper on Capillary Attraction, by J. Ivory, Esq. F.R.S., was read. This able mathematician observed that, notwithstanding the immense number of dissertations on this phænomenon which had been published, still much is wanting towards a full and satisfactory solution of the question. In noticing the various methods proposed for explaining capillary attraction, he remarked that Clairaut was the only philosopher who supposed that the attraction of tubes extended to any visible distance; on the contrary, Newton and all other mathematicians considered the sphere of attraction to be confined to the particles immediately in contact with the tube, and that it did not extend to any sensible distance. This has been the generally received doctrine, and the author thinks it confirmed by the reflection of light, as observed in glass. But the true theory of this phænomenon was not known till Professor Leslie, in the Philosophical Magazine for 1802, first stated, in a popular manner, that the attraction is in proportion to the density of the fluid, and that temperature considerably modifies capillary attraction. This discovery Mr. Ivory considers as furnishing the means of giving a complete theoretical and experimental explanation of this long debated phænomenon. Having observed that most philosophers had dwelt on the theory, and that very few direct experiments were ever made to ascertain the mode or nature and extent of capillary attraction, he commenced a series of actual experiments on the subject. He measured the curves formed by glass when immersed in a fluid, calculated the molecular attraction of the fluid, &c. and in this manner performed a great variety of experiments; but many more, he observed, may still be made without exhausting the subject. His theoretical method differed from that of Laplace, in being susceptible of application to physical experiment; whereas that of the French mathematician was founded on the calculus. The remainder of this ingenious paper was of a nature not fit for public reading.

March 7. A letter from Dr. Brewster to the President was read, On the Nature of double refracting Crystals, and the Method of communicating this Power to Glass, &c. The new and curious researches of this philosopher having led him to investigate particularly the nature of doubly refracting crystals, he has at length discovered that if two plates of glass 0·30 of an inch thick be bent together, they yield the series of seven colours mentioned by Newton. He found in every case that pressure and thickness imparted to glass the power of polarizing light; pressure also on plates of glass gives them the power of double refraction. By mechanical pressure he could thus communicate to plates of glass, crystals of muriate of soda, &c. the property of doubly refracting crystals.

March 14. In the conclusion of Dr. Brewster's experiments, which were illustrated by figures of the various appearances, &c.

this most successful investigator of "physical optics" observed that the subject of double refraction is still extremely intricate and difficult; and that this phænomenon, like electricity and magnetism, presents numerous facts, which in the present state of our knowledge are yet inexplicable. Crystallization, he thinks, is probably owing to a peculiar fluid of which we have yet no adequate knowledge: but however this may be, the facts and experiments which he has adduced tend to favour the undulatory theory of light, &c.

A paper On the Calculus of Function, by Mr. Babbage, was laid before the Society; but it was of a nature not fit for public reading.

March 21. Sir E. Home, in a short paper, related some experiments tending to prove the effects of medicines on the blood, whether taken into the stomach or injected into the veins. He injected fluids into the jugular vein of dogs, and made some experiments on himself with the *eau médicinale*, and instanced the effect it had on his pulse, which is usually 80 in a minute. He compared the effects with those of mercury taken into the stomach; and the general result appears to be, that poisons injected directly into the blood kill somewhat sooner (although in a similar mode) than if taken into the stomach.

Part of a paper by Dr. T. Thomson was read On Phosphoric Acid, in which the atomic theory is introduced and extended, and some of Dr. Berzelius's opinions controverted.

GEOLOGICAL SOCIETY OF LONDON.

Report of the Council to the General Annual Meeting, February 2, 1816.

In presenting their Report for the year 1815, the Council have pleasure in announcing that the Society has increased in number, that its finances have improved, and that the presents and memoirs transmitted to it since the last anniversary have been both numerous and important.

The following is a statement of the numbers of the Society at the last and present anniversaries.

	Feb. 1815.	with-drawn	since dead	transferred to the foreign class	remain	elected 1815.	Total.
Ordinary Members	220*	4	8	2	206	34	240
Honorary Members	92			5			87
Foreign Members				7		7	14
Total	312						341

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* Printed in the [illegible] of 1815 as 222, but two of the number declined their election.

Of the 240 ordinary members, 14 have compounded for the annual contribution, 166 are subject to the annual contribution, and 60 are exempt from it, as not residing within 20 miles of the metropolis.

The following papers have been read before the Society since the last anniversary.

A Memoir on the Native Oxyd of Uranium	by William Phillips, Esq.
...... Sky	by J. MacCulloch, M.D.
...... Quartz Rock	by the same.
...... the Fossils in Cambridgeshire	by the Woodwardian Prof.
...... the Basalt of Christiania	by Samuel Solly, Esq.
...... the Limestone of Plymouth	by the Rev. R. Hannah, jun.
...... the Mine of Huel Peever, in Cornwall	by John Williams, Esq.
...... the Mineralogy of the South-western part of Somersetshire	by Leonard Horner, Esq.
...... a Bone found in the Lake of Geneva	by F. Berger, M.D.
...... Basalt	by Thomas Hare, Esq.
...... some Specimen from the Isle of Tino, in the Archipelago	by Leonard Horner, Esq.
...... a Geode found at Oakhampton	by the Woodwardian Prof.
...... the Rocks in the vicinity of Duftan	by the Rev. W. Buckland.
...... some Specimens found chiefly in Flanders ..	by Knight Spencer, Esq.
...... an Analysis of Swedish Felspar	by J. S. W. Herschel, Esq.
...... a Plant in Chalcedony ..	by J. MacCulloch, M.D.
...... Native Tellurium from Norway	by Professor Esmark.
...... the Angles of the primitive Crystals of Quartz and Sulphate of Barytes ..	by William Phillips, Esq.
...... the Salt Mines of Cardona in Catalonia	by T. S. Traill, M.D.
...... the Iserine found near Liverpool	by the same.
...... Organic Remains from Weston super mare ..	by G. Cumberland, Esq.

A Memoir on the Geology of a part of Lincolnshire ..	by Edward Bogg, Esq.
...... the Carnelians of the East Indies	by —— Copland, Esq.
...... Coak	by John Taylor, Esq.
...... the Physical Geography of Donegal	by F. Berger, M.D.
...... the Plastic Clay and the Beds which accompany it	by the Rev. W. Buckland.
...... the Rocks in the Neighbourhood of the Island of Salette, and Bombay	by —— Babington, Esq.
...... a Catalogue of Minerals found at St. Helena, now at the East India House	by Samuel Davis, Esq.

A list of the presents continued from the 2d volume of the Society's Transactions will be published in the 3d volume.

The Council are happy to announce that, according to the arrangements they have made, the 3d volume of the Geological Transactions will appear in the course of the present session. It will contribute, they hope, to direct the attention of the public to the rational and useful researches that form the subject of its pages, and to support the reputation which the Society has acquired by the publication of its former volumes. The papers submitted to the public in this volume will show that the authors have continued to act upon the principle of avoiding the discussion of theories which are too systematic and extensive, and of treating geology principally as a science of observation.

The exertions of Mr. Greenough to complete the Geological Map of England have been incessant, and a great part of the work is now engraved.

The Council report with the utmost regret that the President has expressed his wish of not continuing in the chair beyond the period for which he originally undertook that office, and which terminates upon the present anniversary. The assurances of the deep sense of the honour conferred upon him, with which he has accompanied the expression of his wish, will be thought superfluous by those who have witnessed his conduct during the time he has filled the situation, and his unremitting endeavours to promote the interests of the Society.

The junior secretary, finding the time occupied by his official duties incompatible with the claims of other engagements of considerable importance to himself, begs leave to withdraw himself

self for the present from a trust which the members at large would no doubt be very desirous again to commit to his tried activity and abilities. The arrangement of the most interesting department of the Society's cabinet, namely, that which illustrates the natural order of succession of the English strata, is entirely due to his care and knowledge. The vacancy occasioned by his retirement, under a less prosperous state of the Society, it would not have been easy in an adequate manner to supply.

It was reported at the last Annual Meeting, that the apartments then in the occupation of the Society were altogether insufficient for the arrangement of the collection; and the Council, not unmindful of this evil, has laboured during the whole of the year to obtain a residence that would meet the growing wants of the Society. The necessity of a removal has since become the more urgent. Notice has been given that the Society intends to quit its present apartments, and it is now a tenant only by sufferance.

A regard to the limited revenues of the Society, and a disinclination on the part of the Council to call upon the members for sacrifices, which though intended to be voluntary might by some be regarded as compulsory, have been the real obstacles to the attainment of this great object. On resigning its trust for the year 1815, as the last act of its power, the Council strongly and earnestly recommends the case to the consideration of the members.

At the Annual General Meeting, held 2d February 1816, the Report of the Council was read and approved;

The thanks of the Society were voted to William Blake, Esq. retiring from the office of President; to George Bellas Greenough, Esq. retiring by rotation from the office of Vice President; to Henry Warburton, Esq. retiring from the office of Secretary; to Samuel Solly, Esq. retiring from the office of Foreign Secretary; to William Hasledine Pepys, Esq. retiring from the office of Treasurer.

FRENCH SOCIETY FOR THE ENCOURAGEMENT OF ARTS.

Among the novelties exhibited by French artists during the year 1815, we find the following enumerated as the most striking:

1. Some beautiful specimens of painting on velvet, by M. Vauchelet; among these was a highly-finished portrait of the emperor Alexander.

2. Several utensils of platina, among which are dish covers, watch chains, &c. by M. Janets, who has entirely suppressed the use of arsenic in his process for rendering platina malleable.

3. Celestial and terrestrial globes of glass for lamps, most accurately engraved and coloured.

Among the printed papers sent in to the Society were:

A paper by M. Lasteyrie, on making flour and bread from potatoes; by which it appears that this vegetable may be kept for many years in the state of powder.

A paper by Baron de Werneck, showing the quantity of potash contained in the various trees, shrubs, and plants, which yield this substance.

XLIX. *Intelligence and Miscellaneous Articles.*

STATE OF CHEMICAL SCIENCE ON THE CONTINENT.

Extract of a Letter from M. DOBEREINER *to M.* VAN MONS*.

Jena, 23d February.

IN a note to p. 59 of my Elements of Pharmaceutical Chemistry, I expressed a suspicion that phosphorus might be composed of a peculiar substance and of hydrogen. This idea, which was founded upon a particular experiment, has since been confirmed.

If we introduce into a retort three parts of iodine and one of phosphorus, both as dry as possible, and heat them by the flame of a spirit lamp, the two bodies will penetrate with an extrication of light and vaporisation of hydro-iodic acid, and there will remain in the retort a brilliant substance of a brownish-red colour, which, when washed and dried, keeps in the air without being altered at the habitual temperature of that fluid: by a strong heat, however, it inflames and burns in the air, dries, and forms vapour of phosphoric acid. As the hydro-iodic acid is the product of the combination of hydrogen with iodine, and as the latter no more than phosphorus contains water, we ought to admit that the hydrogen has taken its origin from the phosphorus, and that the reddish-brown residue, after being washed and dried, is de-hydrogenated phosphorus. This conclusion is also confirmed by the circumstance, that the new iodine, heated with the residue, gives no longer hydro-iodic acid. According to this result, phosphorus is, like sulphur, a hydrine and not a simple body.

The charcoal of wood is a proto-hydrine. When the substance in very fine powder, and mixed with two parts of iron reduced, and one part of oxide of manganese, is made red hot for several hours, the latter of these bodies de-hydrogenates it, and the former condenses it. The produce of this operation is iron alloyed with manganese, and a lamellated friable matter, which is of a blackish-gray colour, and has the metallic lustre. This is carbonion united with a little iron and manganese, from which we may separate it by a long-continued digestion with aqua regia and muriatic acid. The sulphur does not take the latter metals from the fire; which proves that they are chemically combined with the carbonion.

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* [illegible]

In the course of my lecture yesterday I tried to produce borium by treating borax with charcoal, and the experiment completely succeeded. I mixed 220 grains of calcined borax with 18 grains of very fine charcoal, and I kept this mixture for two hours at a red heat in a gun-barrel. There was produced a black fused mass; which after having been washed with water, and afterwards dried, left a deep olive-coloured powder, which had all the characters and all the properties ascribed by Davy to borium.

I discovered nearly two months since a new sparkling pyrophorus, which preserves its property of shining for a long time, and which may serve as a portable match. We obtain it by calcining during an hour at a gentle red heat, and in a gun barrel, a mixture of one part of calcined alum, and two parts of subcarbonate of potash, and from one-half to a whole part of lampblack. This pyrophorus seems to be composed of potassion and sulphuret of carbonion. I also met the sulphuret of carbonion in the liver-formed ore of mercury of Idria, as may be seen in Schweiger's Journal.

I have also recently discovered that the muriate of lead of Derbyshire is composed of phosphoric acid and of lead, and that the *ceta camit* of Peru consists of chloric acid and copper.

M. Dulong, continuing the researches of Thomson, has found that the oxalic acid in uniting warm with the oxides of zinc or lead, allows 0·20 to escape of its weight in water: the oxalates resulting from it do not afterwards yield in their decomposition in the dry way any thing but gaseous oxide of carbon, carbonic acid, and the oxide of the metal. M. Dulong asks if these salts are carbonites or carbonates of reduced metal? He thinks they are the latter. The oxalates of other metals do not give out any water except at a very strong heat; afterwards they yield carbonic acid; no oxide of carbon, but, in lieu of it, the metal reduced: and the oxalates of soluble earth give out, under the action of heat, empyreumatic oil, water, oxide of carbon, carbonated hydrogen, carbonic acid, carbon, and sub-carbonate. We see that these differences depend on the retentive forces or on the energy of the oxides which form the bases of the salts.

It is long since I asserted that the vegetable acids were hydrates, sub-hydrates, or super-hydrates of carbonous acid. This acid exists no more in the uncombined state than the oxide of sulphur, and it resolves, on being isolated, into complete acid and oxide; as the oxide of sulphur resolves itself into sulphur and sulphurous acid; and we may rigorously suppose that the one is a combination of carbonic acid with the oxide of carbon, and the other a combination of sulphurous acid with sulphur, formed both into salts. The weak oxides merely retain the carbonous acid long enough to strip it of its water, and afterwards they give up their oxygen to it, which is saturated into carbonic acid. The strongest oxides separate it instantly from its water; and as they do not give their oxygen to it, the carbonous acid on quitting them is resolved into oxide of carbon and carbonic acid; and the oxides which retain it strongly, as well as its water, allow its oxygen to concentrate into a portion of its substance, and in such a manner that carbon is set free, and hydrogen is displaced from it, by the water: and this principle may form carbonated hydrogen and empyreumatic oil. M. Dulong was thinking of my metallofluores and metallochlores when he supposed it possible that reduced metals could be united to carbonic acid: but the case is very different; for the carbonic acid retains its own water, and deposits only the water of its organization; whereas the fluoric and muriatic acids deposit the first water, and unite dry with the reduced metals. The results of Dulong show that the oxides in the carbonates are proportioned to the quantity of oxygen belonging to the dry carbonic acid, which converts the carbonous acid into common carbonic acid.

Count Real, who lately spent a few days with me, has invented an instrument for the extraction in the cold way of substances of the organic kingdoms, which is very ingenious, simple, and convenient. The dissolving liquid, which sometimes becomes a displacing liquid, produces its own action. The instrument is a cylinder of any given size; but it ought to have the power of considerable resistance. It may be made of glass, wood, tin or copper, according to the use for which it is intended. Two diaphragms, or wire sieves, are fitted in at each extremity, and are intended to contain the substance, which ought to be as minutely pounded as possible, and a little pressed down, that the liquid when entering it may be retained. Over this cylinder a tube may be fixed, from two up to eight feet in height, destined to receive the menstruum, and to produce a strong pressure, which may be rendered sudden or gradual at pleasure. If we fill the tube with mercury, the matter in the cylinder being penetrated with spirits of wine, oil, or water, the pressure becomes immense. This tube may be as narrow as you please, since a liquid, whatever be the base of its column, acts in proportion to the height of the latter, and not in proportion to the diameter of its prolongation. The first portions of liquid which pass through the lower sieve are so charged with the soluble matter, that they are as thick as syrup, and the remaining portions have neither taste nor colour. Quassia, for instance, is obtained at first in its consistence of extract, and what comes afterwards has no taste. The colouring matters with their appropriate solvents are also taken up at once. The extractive matter of tea, coffee, and hops fall off in such a state of concentration, that they may be kept for years without spoiling. They are of course to be diluted with water for use. With a bottle of this extract in one's pocket, it is only necessary to have boiling water, milk and sugar, to make good tea or coffee, which have all their colouring virtues, and have not lost a particle of aroma. If an extract is wanted by means of any other menstruum than water, for instance by alcohol, by alkaline liquor, acid, or oil, the organic substance must be impregnated with them, and water poured in above. These liquids saturated with the soluble substance pass through without the smallest portion of water being mixed with them. Pounded almonds are obtained in thin oil; and, when the water passes, it is of a red colour. For the oils which freeze, the temperature must not be too low. In short, every substance which ought not to be elaborated by heat, or what we may call boiled, is obtained speedily and commodiously by this apparatus, which may be called a *dissolving press*. Its application is universal, and its results will be curious; it will furnish every substance free from alteration by heat, with subcomposition, supercomposition, or decomposition: the analysis of organic substances will be perfected and facilitated by it, and their elements will be soon better known.

Note by M. Van Mons on the above letter.

I have observed to M. Dobereiner, in my reply, that in all probability his iodine was iodous acid, and the water of washing, after having taken off the iodic acid dry, must have left enough of oxygen with the phosphorus to convert it into hydrophosphorous acid. I call iodous acid, iodine half saturated with hydrogen; as we call sulphurous acid, sulphur half saturated with oxygen. We obtain this acid either by demi-saturation with hydrogen, or by the disengagement of the half of the oxygen of the iodine by means of water, or, finally, by the mixture of iodine with iodic acid: and on the supposition that the residue must have been de-hydrogenated phosphorus, this new body, on being burnt in dry air, could not give out vapours of phosphorous acid, and above all hydrophosphorous acid. The oxygen of the iodine remaining with the phosphorus must have been incompletely combined, and the heating by completing the combination must have caused the flame which was developed. Iodine is rarely found without an admixture of idious acid. The iodous acid is perhaps iodate or idionate of iodic acid, and the sulphurous acid, perhaps sulphate of sulphur.

NEW SOUTH WALES.

We have been favoured with the following official account of the most recent topographical surveys of the above important British colony.

" *Government and General Orders.*

" Government-House, Sydney, June 10, 1815.

" CIVIL DEPARTMENT.

" The Governor desires to communicate, for the information of the public, the result of his late tour over the Western or Blue Mountains, undertaken for the purpose of being enabled personally to appreciate the importance of the tract of country lying westward of them; which had been explored in the latter end of the year 1813 and beginning of 1814 by Mr. George William Evans, deputy surveyor of lands.

" To those who know how very limited a tract of country has been hitherto occupied by the colonists of New South Wales, extending along the eastern coast to the north and south of Port Jackson only 80 miles, and westward about 40 miles to the foot of that chain of mountains in the interior which forms its western boundary, it must be a subject of astonishment and regret, that amongst so large a population no one appeared within the first 25 years of the establishment of this settlement possessed of sufficient energy of mind to induce him fully to explore a passage over these mountains :—but, when it is considered that for the greater part of that time even this circumscribed portion of country afforded sufficient produce for the wants of the people, whilst on the other hand the whole surface of the country beyond those limits was a thick and in many places nearly an impenetrable forest, the surprise at the want of effort to surmount such difficulties must abate very considerably.

" The records of the colony only afford two instances of any bold attempt having been made to discover the country to the westward of the Blue Mountains.—The first was by Mr. Bass, and the other by Mr. Caley, and both ended in disappointment—a circumstance which will not be much wondered at by those who have lately crossed those mountains.

" To Gregory Blaxland and William Wentworth, esquires, and Lieutenant Lawson, of the Royal Veteran Company, the merit is due of having, with extraordinary patience and much fatigue, effected the first passage over the most rugged and difficult part of the Blue Mountains.

"The governor, being strongly impressed with the importance of the object, had, early after his arrival in this colony, formed the resolution of encouraging the attempt to find a passage to the western country, and willingly availed himself of the facilities which the discoveries of these three gentlemen afforded him. Accordingly, on the 20th of November 1813 he intrusted the accomplishment of this object to Mr. George William Evans, deputy surveyor of lands, the result of whose journey was laid before the public, through the medium of the Sydney Gazette, on the 12th of February 1814.

"The favourable account given by Mr. Evans of the country he had explored, induced the governor to cause a road to be constructed for the passage and conveyance of cattle and provisions to the interior; and men of good character, from amongst a number of convicts who had volunteered their services, were selected to perform this arduous work, on condition of being fed and clothed during the continuance of their labour, and being granted emancipations as their final reward on the completion of the work.

"The direction and superintendence of this great work was intrusted to William Cox, esq. the chief magistrate at Windsor; and to the astonishment of every one who knows what was to be encountered, and sees what has been done, he effected its completion in six months from the time of its commencement, happily without the loss of a man, or any serious accident. The governor is at a loss to appreciate fully the services rendered by Mr. Cox to this colony, in the execution of this arduous work, which promises to be of the greatest public utility, by opening a new source of wealth to the industrious and enterprising. When it is considered that Mr. Cox voluntarily relinquished the comforts of his own house, and the society of his numerous family, and exposed himself to much personal fatigue, with only such temporary covering as a bark hut could afford from the inclemency of the season, it is difficult to express the sentiments of approbation to which such privations and services are entitled.

Mr. Cox having reported the road as completed on the 21st of January, the governor, accompanied by Mrs. Macquarie and that gentleman, commenced his tour on the 25th of April last, over the Blue Mountains, and was joined by Sir John Jamieson at the Nepean, who accompanied him during the entire tour.—The following gentlemen composed the governor's suite: Mr. Campbell, sectretary; Capt. Antill, major of brigade; Lieut.

Watts, aid-de-camp; Mr. Redfern, assistant surgeon; Mr. Oxley, surveyor general; Mr. Meehan, deputy surveyor general; Mr. Lewin, painter and naturalist; and Mr. G. W. Evans, deputy surveyor of lands, who had been sent forward for the purpose of making further discoveries, and rejoined the party on the day of arrival at Bathurst Plains.

" The commencement of the ascent from Emu Plains to the first depôt, and thence to a resting place, now called Spring Wood, distant 12 miles from Emu Ford, was through a very handsome open forest of lofty trees, and much more practicable and easy than was expected. The facility of the ascent for this distance excited surprise, and is certainly not well calculated to give the traveller a just idea of the difficulties he has afterwards to encounter.—At a further distance of four miles a sudden change is perceived in the appearance of the timber and the quality of the soil—the former becoming stunted, and the latter barren and rocky. At this place the fatigues of the journey may be said to commence. Here the country became altogether mountainous, and extremely rugged.—Near to the 18th mile mark (it is to be observed that the measure commences from Emu Ford) a pile of stones attracted attention: it is close to the line of road, on the top of a rugged and abrupt ascent, and is supposed to have been placed there by Mr. Caley, as the extreme limit of his tour:—hence the governor gave that part of the mountain the name of Caley's Repulse. To have penetrated even so far, was at that time an effort of no small difficulty.—From hence, forward to the 26th mile, is a succession of steep and rugged hills, some of which are almost so abrupt as to deny a passage altogether; but at this place a considerably extensive plain is arrived at, which constitutes the summit of the Western Mountains; and from thence a most extensive and beautiful prospect presents itself on all sides to the eye. The town of Windsor, the river Hawkesbury, Prospect Hill, and other objects within that part of the colony now inhabited, of equal interest, are distinctly seen from hence.—The majestic grandeur of the situation, combined with the various objects to be seen from this place, induced the governor to give it the appellation of The King's Table Land.—On the SW. side of the King's Table Land the mountain terminates in abrupt precipices of immense depth, at the bottom of which is seen a [illegible] as romantically beautiful as can be imagined, bounded on the further side by mountains of great magnitude, terminating equally abruptly as the others; and the whole thickly covered with timber. The length of this picturesque and remarkable tract of country is about 24 miles, to which the governor gave the name of The Prince Regent's Glen.—Proceeding hence to the 33d mile on the top of a hill, an opening presents itself on the SW. side of the Prince Regent's Glen, from whence a view is obtained particularly beautiful and grand—mountains rising beyond mountains, with stupendous masses of rock in the fore-ground, here strike the eye with admiration and astonishment. The circular form in which the whole is so wonderfully disposed, induced the governor to give the name of Pitt's Amphitheatre (in honour of the late right honourable William Pitt) to this offset or branch from the Prince Regent's Glen. The road continues from hence, for the space of 17 miles, on the ridge of the mountain which forms one side of the Prince Regent's Glen, and there it suddenly terminates in nearly a perpendicular precipice of 676 feet high, as ascertained by measurement. The road constructed by Mr. Cox down this rugged and tremendous descent, through all its windings, is no less than three-fourths of a mile in length, and has been executed with such skill and stability as reflects much credit on him. The labour here undergone, and the difficulties surmounted, can only be appreciated by those who view this scene. In order to perpetuate the memory of Mr. Cox's services, the governor deemed it a tribute justly due to him, to give his name to this grand and extraordinary pass; and he accordingly called it Cox's Pass. Having descended into the valley at the bottom of this pass, the retrospective view of the overhanging mountain is magnificently grand. Although the present pass is the only practicable point yet discovered for descending by, yet the mountain is much higher than those on either side of it, from whence it is distinguished at a considerable distance, when approaching it from the interior, and in this point of view it has the appearance of a very high distinct hill, although it is in fact only the abrupt termination of a ridge. The governor gave the name of Mount York to this termination of the ridge, in honour of his royal highness the duke of York.

" On descending Cox's Pass, the governor was much gratified by the appearance of good pasture land and soil fit for cultivation, which was the first he had met with since the commencement of his tour. The valley at the base of Mount York he called The Vale of Clwyd, in consequence of the strong resemblance it bore to the vale of that name in North Wales. The grass in this vale is of a good quality and very abundant, and a rivulet of fine water running along it from the eastward, which unites itself at the western extremity of the vale with another rivulet containing still more water.—The junction of these two streams forms a very handsome river, now called by the governor Cox's River; which takes its course, as has been since ascertained, through the Prince Regent's Glen, and empties itself into the river Nepean; and it is conjectured, from the nature of the country through which it passes, that it must be one of the principal causes of the floods which have been occasionally felt on the low banks of the river Hawkesbury, into which the Nepean discharges itself. The vale of Clwyd, from the base of Mount York, extends six miles in a westerly direction, and has its termination at Cox's River. Westward of this river the country again becomes hilly, but is generally open forest land, and very good pasturage.

"Three miles to the westward of the Vale of Clwyd, Messrs. Blaxland, Wentworth, and Lawson had formerly terminated their excursion; and when the various difficulties are considered which they had to contend with, especially until they had effected the descent from Mount York, to which place they were obliged to pass through a thick brush-wood, where they were under the necessity of cutting a passage for their baggage-horses, the severity of which labour had seriously affected their healths, their patient endurance of such fatigue cannot fail to excite much surprise and admiration.—In commemoration of their merits, three beautiful high hills joining each other at the end of their tour at this place, have received their names in the following order; viz. — Mount Blaxland, Wentworth's Sugar Loaf, and Lawson's Sugar Loaf. A range of very lofty hills and narrow valleys alternately form the tract of country from Cox's River, for a distance of 16 miles, until the Fish River is arrived at; and the stage between these rivers is consequently very severe and oppressive on the cattle. To this range the governor gave the name of Clarence Hilly Range.

"Proceeding from the Fish River, and at a short distance from it, a very singular and beautiful mountain attracts the attention, its summit being crowned with a large and very extraordinary-looking rock, nearly circular in form, which gives to the whole very much the appearance of a hill fort, such as are frequent in India.—To this lofty hill Mr. Evans, who was the first European discoverer, gave the name of Mount Evans. Passing on from hence the country continues hilly, but affords good pasturage, gradually improving to Sidmouth Valley, which is distant from the pass of the Fish River eight miles. The land here is level, and the first met with unencumbered with timber: it is not of very considerable extent, but abounds with a great variety of herbs and plants, such [illegible] would probably highly interest and gratify the scientific botanist.—This beautiful little valley runs north-west and south-east, between hills of easy ascent, thinly covered with timber.—Leaving Sidmouth Valley, the country becomes again hilly, and in other respects resembles very much the country to the eastward of the valley for some miles. Having reached Campbell River, distant 13 miles from

Sidmouth Valley, the governor was highly gratified by the appearance of the country, which there began to exhibit an open an extensive view of gently rising grounds and fertile plains.—Judging from the height of the banks, and its general width, the Campbell River must be on some occasions of very considerable magnitude; but the extraordinary drought which has apparently prevailed on the western side of the mountains, equally as throughout this colony for the last three years, has reduced this river so much that it may be more properly called a chain of pools than a running stream at the present time. In the reaches or pools of the Campbell River, the very curious animal called the paradox, or water-mole, is seen in great numbers. The soil on both banks is uncommonly rich, and the grass is consequently luxuriant.—Two miles to the southward of the line of road which crosses the Campbell River, there is a very fine rich tract of low lands, which has been named Mitchell Plains. Flax was found here growing in considerable quantities.—The Fish River, which forms a junction with the Campbell River a few miles to the northward of the road and bridge over the latter, has also two very fertile plains on its banks, the one called O'Connell Plains, and the other Macquarie Plains, both of considerable extent, and very capable of yielding all the necessaries of life.

" At the distance of seven miles from the bridge over the Campbell River, Bathurst Plains open to the view, presenting a rich tract of champaign country of 11 miles in length, bounded on both sides by gently rising and very beautiful hills, thinly wooded. The Macquarie River, which is constituted by the junction of the Fish and Campbell River, takes a winding course through the plains, which can be easily traced from the high lands adjoining, by the particular verdure of the trees on its banks, which are likewise the only trees throughout the extent of the plains.—The level and clean surface of these plains gives them at first view very much the appearance of lands in a state of cultivation.

" It is impossible to behold this grand scene without a feeling of admiration and surprise, whilst the silence and solitude which reign in a space of such extent and beauty as seems designed by Nature for the occupancy and comfort of man, create a degree of melancholy in the mind which may be more easily imagined than described.

" The governor and suite arrived at these plains on Thursday the 4th of May, and encamped on the southern or left bank of the Macquarie River—the situation being selected in consequence of its commanding a beautiful and extensive prospect for many miles in every direction around it.—At this place the governor remained for a week, which time he occupied in making excursions in different directions through the adjoining country, on both sides of the river.

"On Sunday, the 7th of May, the governor fixed on a site suitable for the erection of a town at some future period, to which he gave the name of Bathurst, in honour of the present secretary of state for the colonies.—The situation of Bathurst is elevated sufficiently beyond the reach of any floods which may occur, and is at the same time so near to the river on its south bank as to derive all the advantages of its clear and beautiful stream. The mechanics and settlers of whatever description who may be hereafter permitted to form permanent residences to themselves at this place, will have the highly important advantages of a rich and fertile soil, with a beautiful river flowing through it, for all the uses of man. The governor must however add, that the hopes which were once so sanguinely entertained, of this river becoming navigable to the Western Sea, have ended in disappointment.

"During the week that the governor remained at Bathurst, he made daily excursions in various directions: one of these extended 22 miles in a south-west direction, and on that occasion, as well as on all the others, he found the country composed chiefly of valleys and plains, separated occasionally by ranges of low hills;—the soil throughout being generally fertile, and well circumstanced for the purpose of agriculture or grazing.

"The governor here feels much pleasure in being enabled to communicate to the public, that the favourable reports which he had received of the country to the west of the Blue Mountains have not been by any means exaggerated,—the difficulties which present themselves in the journey from hence are certainly great and inevitable; but those persons who may be inclined to become permanent settlers there, will probably content themselves with visiting this part of the colony but rarely, and of course will have them seldom to encounter.—Plenty of water and a sufficiency of grass are to be found in the mountains for the support of such cattle as may be sent over them; and the tracts of fertile soil and rich pasturage which the new country affords, are fully extensive enough for any increase of population and stock which can possibly take place for many years.

"Within a distance of ten miles from the site of Bathurst, there is not less than fifty thousa[illegible]res of land clear of timber, and fully one half of that may b[illegible]sidered excellent soil, well calculated for cultivation. It is a matter of regret, that in proportion as the soil improves the timber degenerates; and it is to be remarked, that every where to the westward of the mountains it is much inferior both in size and quality to that within the present colony; there is, however, a sufficiency of timber of tolerable quality within the district around Bathurst, for the purposes of house-building and husbandry.

"The governor has here to lament, that neither coals nor limestone have been yet discovered in the western country; articles in themselves of so much importance, that the want of them must be severely felt whenever that country shall be settled.

"Having enumerated the principal and most important features of this new country, the governor has now to notice some of its live productions. All around Bathurst abounds in a variety of game; and the two principal rivers contain a great quantity of fish, but all of one denomination, resembling the perch in appearance, and of a delicate and fine flavour, not unlike that of a rock cod: this fish grows to a large size, and is very voracious. Several of them were caught during the governor's stay at Bathurst, and at the halting-place on the Fish River. One of those caught weighed 17 lbs. and the people stationed at Bathurst reported that they had caught some weighing 25 lbs.

"The field game are the kangaroos, emus, black swans, wild geese, wild turkeys, bustards, ducks of various kinds, quail, bronze, and other pigeons, &c. &c. The water-mole, or paradox, also abounds in all the rivers and ponds.

"The site designed for the town of Bathurst, by observation taken at the flag-staff, which was erected on the day of Bathurst receiving that name, is situated in latitude 33° 24′ 30″ south, and in longitude 149° 37′ 45″ east of Greenwich, being also 27½ miles north of Government-house in Sydney, and 94½ west of it, bearing west 20° 30′ north, 83 geographic miles, or 95½ statute miles; the measured *road* distance from Sydney to Bathurst being 140 English miles.

"The road constructed by Mr. Cox and the party under him commences at Emu Ford, on the left bank of the river Nepean, and is thence carried 101½ miles to the flag-staff at Bathurst: this road has been carefully measured, and each mile regularly marked on the trees growing on the left side of the road proceeding towards Bathurst.

"The governor in his tour made the following stages, in which he was principally regulated by the consideration of having good pasturage for the cattle, and plenty of water:

1st stage:—Spring Wood[illegible]tant from Emu Ford	12 miles
2d ditto—Jamieson's Va[illegible]r 2d depôt, distant from ditto	28 miles
3d ditto—Blackheath, distant from ditto ..	41 miles
4th ditto—Cox's River, distant from ditto ..	56 miles
5th ditto—The Fish River, distant from ditto ..	72 miles
6th ditto—Sidmouth Valley, distant from ditto ..	80 miles

7th stage—Campbell River, distant from Emu Ford 91 miles
8th ditto—Bathurst, distant from ditto .. .. .. 101½ miles.

"At all of which places the traveller may assure himself of good grass, and water in abundance.

"On Thursday the 11th of May the governor and suite set out from Bathurst on their return, and arrived at Sydney on Friday the 19th ultimo.

"The governor deems it expedient here to notify to the public, that he does not mean to make any grants of land to the westward of the Blue Mountains until he shall receive the commands of His Majesty's ministers on that subject, and in reply to the report he is now about to make them upon it.

"In the mean time, such gentlemen or other respectable free persons as may wish to visit this new country, will be permitted to do so on making a written application to the governor to that effect; who will order them to be furnished with written passes. It is at the same time strictly ordered and directed, that no person, whether civil or military, shall attempt to travel over the Blue Mountains without having previously applied for and obtained permission, in the above prescribed form. The military guard stationed at the first depôt on the mountains will receive full instructions to prevent the progress of any persons who shall not have obtained regular passes. The necessity for the establishing and strictly enforcing this regulation is too obvious to every one who will reflect on it, to require any explanation here.

"The governor cannot conclude this account of his tour, without offering his best acknowledgements to William Cox, esq. for the important service he has rendered to the colony in so short a period of time, by opening a passage to the new-discovered country, and at the same time assuring him, that he shall have great pleasure in recommending his meritorious services on this occasion to the favourable consideration of His Majesty's Ministers.

"By command of his excellency the Governor
"John Thomas Campbell, Secretary."

M. Orfila has continued his interesting researches upon poisons. According to the second part of his work now published, he does not consider opium as a narcotic or a stimulant, but as exercising an action completely *[illegible]eneris.* With respect to the *Solanum nigrum*, M. Orfila [illegible] not think it is poisonous at all; and adds, that the *Belladonna* must have been the deleterious plant mistaken for it by authors.

With respect to the effects of poison from narcotics, as they have been called, M. Orfila observes that authors have prescribed the vegetable acids, coffee, camphor, water, chlorine, and bleeding. M. Orfila has proved, however, 1. That the vegetable acids constantly hasten death, when they exist in the stomach along with the narcotic, which is owing to the acids forming the solution of the poison, and consequently its absorption. (There are ten experiments to verify this fact.) 2. That acidulated water was very useful for combating the effects of narcotics, when they had been previously rejected by vomiting: thus animals which would have died infallibly at the end of an hour, were saved by administering to them night and day, for twenty-four or thirty-six hours, several doses of water soured by a little vinegar: those which were nearly revived by the end of the day, and which had been neglected during the night, died for want of assistance. 3. That a strong infusion of coffee is an antidote to the effects of poison by means of narcotics, and the animals to which it was administered night and day recovered. 4. That the decoction of coffee is much less energetic than the infusion. 5. That camphor is not the counterpoison to narcotics, but that it may be administered in small doses to diminish their effects. 6. That water and mucilaginous preparations, so far from being useful, hasten the approach of death, because they favour the absorption of the poison. 7. That bleeding was never injurious, and that it was frequently sufficient to operate the revival of plethoric animals, which would nevertheless have died two or three days afterwards, if they had not been attended to: and lastly, that it is best always to open the jugular vein. 8. That chlorine acts nearly like the vegetable acids.

M. Orfila has announced his intention to compare the effects of the poisonous plants of Africa and the South of Europe, with those which he has obtained in France. He is also collecting materials for a work, in which he means to show in what cases the fluids of living animals are deranged, become venomous, and the diseases which they produce.

Mr. Robertson Buchanan, of Glasgow, has published a work On the best Method of constructing and navigating Steam Boats, illustrated with fourteen engravings.

Madame Candida Lena Perpenti, of Como in Italy, has revived the art of spinning [illegible] weaving the *amianthus*. M. Moscati, of Turin, has sent [illegible] good specimens of the cloth made from it, with a paper descriptive of the process, to the French Society of Arts.

The cloth manufactory at Mouzon in the Ardennes, in France, has been lighted since 1813 with gas made from coal.

M. Vogel has analysed the brick-dust sediment of the human urine, known by the appellation of the *rosacic* acid. This substance has not been found in the urine of a healthy person by M. Vogel, although M. Proust was of a contrary opinion. The former observes:—" I never found that this red substance was formed previous to or during the state of fever; it always began to make its appearance at the period when the crisis was completely past. Recently I had an opportunity of procuring a considerable quantity of this substance. A friend who is subject to gouty attacks voided this kind of urine during fifteen days, and collected the red powder for me by filtration. Cold water has no effect on it, but boiling water dissolves it completely. The solution is brownish; and as it cools, a white powder is deposited. The liquid has a smell like that of urine, and reddens turnsole tincture.

" Boiling alcohol of the specific gravity of 40° in Baumé's areometer, dissolves it sensibly, but not so completely as boiling water. By decanting off the alcohol, and boiling the residue several times with a new quantity of alcohol, this liquid finishes by being perfectly colourless, and there remains a much paler powder, and upon which alcohol seems no longer to act. After being dried, this powder is almost white. It forms a strong froth with cold nitric acid; and when the mixture is evaporated to dryness, red soft scales remain, as is the case when uric acid is heated by nitric acid. The alcohol saturated with this red matter was evaporated to dryness, and a red powder remained unalterable in the air, which I consider as pure rosacic acid, and from which the uric acid has been separated by the alcohol. On the rosacic acid thus purified I made the following experiments:—It was completely dissolved in water: the aqueous solution reddens turnsole tincture, without however disturbing lime-water; which proves that no phosphoric acid is present.

" The rosacic acid dissolves without effervescence in concentrated sulphuric acid. A red liquor is produced, which gradually becomes darker. This liquid loses its colour on the addition of a little water, and a white powder is precipitated. The same white sediment is produced by alcohol. The white powder is almost insoluble in water, when it is washed until all the sulphuric acid is taken from it. It p[illegible] all the characteristics of uric acid. When we sprinkle [illegible]osacic acid with a few drops of sulphuric acid, the powder acquires a fine red colour; but it soon becomes white, and in this state it resembles the uric acid.

" The liquid sulphurous acid in which we shake the pulverulent acid acquires a very lively red; a shade which it preserves a long time even in sulphurous acid, and without the latter losing its peculiar smell. When the rosacic acid is dried, which has been in contact with the sulphurous acid, it yields a very fine carmine colour. When nitric acid at 32° is poured on the rosacic acid, there is immediately a considerable swelling up, and a brisk effervescence of nitrous gas; the red powder disappears, and a yellowish white substance is formed. On boiling the liquor, the whole is dissolved, and there remain, by a slow evaporation, red scales perfectly simil[illegible] to those which are obtained by treating the uric acid with nitric acid. According to M. Proust, by pouring nitric acid on this acid a considerable quantity of carbonic acid gas is liberated. Since the nitric acid only can produce such an effervescence, the extrication of the carbonic acid and of the nitrous gas can only be ascribed to a reciprocal decomposition which the rosacic acid and the nitric acid exercise on each other. The simple muriatic acid does not appear to have a sensible action on the rosacic acid; the powder remains in it diluted, without losing its intensity of colour, and it is only after a few days that it becomes fawn-coloured. The oxy-muriatic acid discolours the red colour very speedily, and makes it yellow.

"Water charged with sulphurated hydrogen has no kind of action on the rosacic acid. These two substances may remain together for fifteen days without undergoing any change. Nevertheless, after a longer time the red powder disappears entirely, and the liquor acquires a putrid ammoniacal smell.

"When we sprinkle the rosacic acid with a concentrated solution of caustic potash, the powder immediately acquires a brownish colour, and abundance of ammonia is liberated. This combination of rosacic acid and of potash is very soluble in water.

"The acids precipitate from it a powder of a yellow colour; and it would seem as if the rosacic acid by its union with potash had already undergone a kind of decomposition; at least I have not been able to reproduce it by means of an acid with its primitive red colour. Liquid ammonia left in contact during some hours with the rosacic acid converts it into a fine yellow powder. The ammonia is combined in this yellow powder in the state of salt with the rosacic acid, and this salt is more soluble in water than the rosacic acid itself. [illegible]osacic acid is precipitated in a yellow powder from the aq[illegible]s solution of this salt with a base of ammonia. On sprinkling the rosacic acid with a concentrated solution of nitrate of silver, the powder loses its colour in a few hours and becomes bottle green. The pure nitric acid diluted in a solution of nitrate of silver also assumes after some time a brownish aspect. The nitrate of mercury and muriate of tin produce nothing similar on the rosacic acid. Upon the whole, M. Vogel concludes, with the exception of the circumstance of colour, and of the action of the sulphurous and sulphuric acids, that the rosacic acid does not differ essentially from the uric acid; and nature, in changing the one into the other, makes no great effort."

PHOSPHATE OF ALUMINE.

M. Vauquelin has published in the *Annales de Chimie* the following brief note on the phosphate of alumine:—"The method hitherto regarded as the best for separating the phosphoric acid from iron, with which it is frequently mixed in the ores, consists in fusing the latter with potash, &c.; but if there is at the same time alumine in these ores, it is also dissolved in the alkali, and is found united to the phosphoric acid when we precipitate the latter, and increases the quantity of it. This alumine might make us believe in the presence of the phosphoric acid, even when it does not exist, if we do not examine the precipitate with attention.

"If the alumine exists with the phosphoric acid in an ore of iron, it is evident that these two bodies will be dissolved in the potash, will be precipitated from it when we saturate the alkali precisely by an acid, and will be redissolved together by an excess of acid. If we add lime water in order to precipitate the phosphoric acid, the alumine will be also precipitated; but if we treat the precipitate when still moist by a solution of potash, it will not be completely dissolved; and this will be the proof of the existence of the phosphoric acid: otherwise the solution would take place completely.

"This method appears to me the most certain, not only for ascertaining the presence of the phosphoric acid in iron ores, but also for estimating the quantity of it. In fact, we cannot analyse the phosphate of alumine either by the alkalies or the carbonates: the former dissolve the entire combination, the others dissolve it in part; in such a way, however, that there is a greater quantity of phosphoric acid in the part dissolved than in that which is not. I ascertained this in the following way: I boiled a [illegible]tain quantity of phosphate of alumine with a solution of carbonate of potash. I filtered the liquor in order to separate it from the undissolved portion, and [illegible]turated with acetic acid the excess of the carbonate of potas[illegible] there was formed a precipitate, which was phosphate of alumine. I afterwards put an excess of acid in the liquor, and I assured myself that in this state it was not precipitated by ammonia; a proof that it did not contain any more phosphate of alumine: but it had been precipitated by lime water; which proves that the alkali had divided the phosphate of alumine into super-sulphate which it had dissolved, and into sub-sulphate which it left. We may besides distinguish pure alumine from the phosphate with this base: the alumine is transparent, and as it were gelatinous; the phosphate on the contrary is opaque white: but this quality does not always announce the presence of the phosphoric acid in alumine, for silex and lime give it this opaque aspect.

"Although ammonia does not perceptibly dissolve pure alumine, it dissolves a great quantity of phosphate of alumine, which it shares, like the carbonates, both into super- and sub-sulphate."

M. Vauquelin has analysed the bark of the South American plant called the *malambo*, brought to Europe by Messrs. Humboldt and Bonpland. It had been strongly recommended as a febrifuge, and of service as a substitute for the Peruvian bark now in use. M Vauquelin finds the malambo bark to contain: 1. a volatile aromatic oil; 2. a very bitter resin; 3. an extract soluble in water. The resin is of a reddish brown, dry, and shining in its fracture: when put into the mouth it seems to be at first tasteless, but some time afterwards its bitterness is developed in a very striking manner: it is very soluble, particularly in warm alcohol, and its solution is abundantly precipitated by water; it is not soluble in the alkalies. When placed upon a warm body, it is dispersed almost entirely in smoke which has the smell of incense. When subjected to the operation of heat in close vessels, it furnished an acid water and a thick oil the smell of which was not agreeable, and some charcoal.

The extract is of a yellowish brown colour: its fracture is shining when dry, but it becomes soft in the air; it is not bitter if it is well washed in alcohol: it is viscous and gluey when humid. When subjected to heat in close vessels this extract furnishes a brown oil, a watery mixture which reddens turnsole, and from which potash nevertheless liberates ammonia in a very sensible manner. The charcoal remaining in the retort, when burnt in a platina crucible left some very alkaline ashes, and which furnished by lixiviation a considerable quantity of subcarbonate of potash of a green colour, similar to that of certain potashes of commerce. This colour is owing to manganese, for by saturating this alkali by the muriatic acid the combination assumed a very fine r[illegible]olour. This alkali certainly comes from some salts insolub[illegible]alcohol, such as the tartrate, citrate, or oxalate of potash.

The volatile oil is slightly citrine, lighter than water, of a smell which seems at once to resemble pepper and thyme: it is slightly soluble in water, to which it communicates its smell and its pungent taste: it is very soluble in alcohol.

The bark of the malambo when incinerated furnished yellowish white ashes, which were entirely dissolved with effervescence in the muriatic acid: the ammonia precipitated from its solution a little phosphate of lime mixed with iron; the sulphuric acid afterwards poured into this solution, and the latter evaporated to dryness and calcined, gave abundance of sulphate of lime, a little sulphate of magnesia, and there was also a little silex This bark therefore contains the same principles as the vegetables of Europe, and shows that the soil is nearly the same over the globe, or at least that vegetables uniformly extract from it the same substances.

The most abundant principle in the malambo bark is the resin, since it forms one-fifth part of it. In this resin the bitter taste resides, and also the chief virtue of the bark. The volatile and aromatic oil which accompanies the bitter principle affords room to hope that it may be employed as a tonic. But the resin being very abundant and nauseously bitter, and the volatile oil very acrid, the bark must be given at first in small doses and with caution. The form most advantageous for administering it is that of tincture, mixed with syrup or water and sugar.

EARTHQUAKE AT THE ISLAND OF MADEIRA.

By letters from Madeira, dated on the 8th of February last, it appears that there was a severe shock of an earthquake felt there on the 2d of February. The shock was very violent, and lasted four or five minutes, according to different persons. It threw down the cross of one of the parish churches, of which the walls were also shaken. Other churches and houses were damaged in various parts of the island. What appears more singular is, that on the 5th of February an American vessel arrived, the captain of which related, that on the 2d, about one o'clock in the morning, being then about 300 miles from the Azores, and 700 miles from Madeira, his vessel sustained a shock as severe as if it had struck on a rock. The crew were greatly alarmed, and the captain sounded immediately, but found no bottom. He was perfectly at a loss how to account for this extraordinary circumstance, until after his arrival at Madeira.

STEAM ENGINES IN CORNWALL.

By Messrs. Leans' Report for Ja[illegible]y, the average work of 33 engines was 20,694,630 pounds of water lifted one foot high with each bushel of coals consumed. Woolf's engine at Wheal Vor during the same month lifted 47,900,333 pounds, and his engine at Wheal Abraham 47,622,040 pounds, one foot high with each bushel of coals.

By the Report for February the average work of 34 engines was 20,667,398 pounds lifted one foot with each bushel of coals. Woolf's engine at Wheal Vor lifted 45,493,303; and the one at Wheal Abraham 45,896,382 pounds one foot high with each bushel.

LIST OF PATENTS FOR NEW INVENTIONS.

To Joseph Manton, of Davies-street, Berkeley-square, gunmaker, for his improvements in the construction and use of certain of the parts of fire-arms, and also of the shoeing of horses. February 29, 1816.—6 months.

To Francis Terril, of Longacre, coachmaker, for his new wheel-guard.—2d March.—6 months.

To John Wood the Younger, of Bradford, worsted spinner, and Joshua Wordsworth, in the parish of Leeds, machine maker, for certain improvements in machines applicable to every description of spinning.—2d March.—2 months.

To Bryan Donkin, of Grange-road, Bermondsey, engineer, for a method of effecting certain purposes or processes, in which a temperature above that of boiling water is requisite or desirable, by applying the temperature requisite or desirable in the said certain process for effecting the said certain purposes, in a manner not hitherto employed therein - 2d March.—2 months.

To George Frederick Muntz, of Birmingham, in the county of Warwick, roller of metals, for a method of abating, or nearly destroying, smoke, and of obtaining a valuable product therefrom.—2d March —2 months

To John Leigh Bradbury, of the city of Gloucester, gentleman, for certain improvements in the machinery for spinning of cotton, flax, wool, tow, worsted, or any other fibrous substance.—9th March.—6 months. -

To Pierre François Montgolfier, of Leicester-square, engineer, for his improvements on the machine denominated *belier hydraulique*, or hydraulic ram.—14th March.—6 months.

To John Stead, of Wicker, in the township of Brightside Bierlow, in the parish of Sheffield, coachmaker, for a stage-coach, or other coach or carriage, for the carrying of passengers on lighter and more commodious principles than usual, that is to say, for the carrying of four or more inside passengers; six, eight, ten, or more outside passengers, with greater safety than those now in use carrying the same n[illegible]er of passengers.—14th March.—2 months.

To Marc Isambard Brunel, of Lindsay-row, Chelsea, who in consequence of a communication made to him by a certain foreigner residing abroad, is become possessed of the invention of the "tricolteur," or knitting machine.—14th March.—6 months.

To William West and Daniel West, both of Bombay in the

East Indies, for a certain method of producing and applying power and motion to presses and other mechanical apparatus.—14th March.—20 months.

To Pierre François Montgolfier, of Leicester-square, engineer, and Henry Daniel Dayme of the same place, gentleman, for certain improvements in a machine which acts by the expansion or contraction of air heated by fire, and which machine is applicable to the raising of water, or giving motion to mills or other machines.—14th March.—6 months.

To James Dowson, of No. 63, Strand, Esq. for certain new or improved means of producing or communicating motion in or unto bodies, either wholly or in part surrounded by water, or any or either of them, by the reaction of suitable apparatus upon the said water, or air, or upon both of them.—14th March.—6 months.

To John Filkin, of No. 60, Old-street Road, in the parish of Shoreditch, truss-maker, William Filkin of the same place, truss-maker, and Joseph Barton, of No. 20, Lombard-street, in the city of London, gentleman, for a new truss.—14th March.—6 months.

To Samuel Jean Pauley, of No. 5, Knightsbridge, opposite the Cannon brewhouse, engineer, for an article or substance for making, without seams, coats, great-coats, waistcoats, habits, cloaks, pantaloons, mantles, stockings, socks, and any other kind of clothing, covers for umbrellas and for hats. Mattresses, seats, and cushions filled with atmospheric air.—23th March.—2 months.

To Emo Tonkin, of the City Road, in the parish of St. Leonard, Shoreditch, for a globe reflecting stove for light and heat.—20th March.—2 months.

To Pierre Pelleton, of Manchester, in the county of Lancaster, chemist, for a new method of making sulphuric acid, commonly called oil of vitriol.—18th March.—6 months.

To Emerson Dawson, of Welbeck-street, ironmonger, and John Isaac Hawkins, of Tichfield-street, for an improvement or addition to grates and stoves, and an instrument, machine, or apparatus, for supplying grates and stoves with fuel.—23d March.—2 months

To Robert Cameron, jun. of the city of Edinburgh, paper-maker, for a machine for manufacturing paper on a principle entirely new.—23 March.—6 months.

To Joseph Bowles, of Bennett's Street, Blackfriars Road, mill-wright, for certain improvements in or on oil mills.—23d March.—6 months.

To Samuel Brown, of Westgate, in the county of Norfolk, iron-founder, for certain improvements upon the swing and wheeled plough-carriages and plough-shares.—23d March.—6 months.

To Henry Osborne, of Bordesley, in the parish of Aston in the county of Warwick, for a method or principle of producing cylinders of various descriptions.—23d March.—6 months.

To John Merryweather, of the castle of Lincoln, in the county of Lincoln, gentleman, for certain means of propelling boats and vessels through the water.—23d March.—6 months.

To Abraham Rogers, of Sheft, in the parish of Halifax, in the county of York, coal merchant, for a method of effecting a saving in the consumption of coal, or fuel, by an improvement in the mode of setting or heating boilers and steam-engines, and other bodies of different descriptions; and also for heating and warming stoves, drying houses, manufactories, and other buildings, and for burning different descriptions of gasses.—23d March.—2 months

To Leberecht Stanhauser, of Old Bond-street, merchant, for a new or improved castor or roller for tables, sofas, bedsteads, and other articles.—23d March.—2 months.

To James Younie, of Theobald's Road, Red-lion-square, ironmonger, for his discovery for the prevention or cure of smoky chimneys.—23d March.—4 months.

Meteorological Observations made principally at Edinburgh in March 1816.

Sir,—We have had the great changeability of the weather during the greatest part of the year in the south of England, so frequently alluded to by meteorologists, that it may be worth while to notice, previous to the regular journal of the weather which I subjoin, that the inhabitants of this city say they do not remember to have had so changeable a season for many years. Since my arrival here on the 23d of February, the atmosphere has been constantly changing; clear frost, snow, rain, and the heat of 50° of Fahrenheit, all in the space of twenty-four hours. I regret that the multiplicity of other employments has prevented me from keeping a register of the weather, till the 12th instant.

March 12.—Rapidly succeeding but gentle showers with clear intervals, and gale from the westward, with temperate atmosphere of about 50° at the maximum.

March 13.—Very clear morning, followed about noon by snow showers, and wind in gales. Fine clear moon-light night, and gentle stratus near the ground.

March 14.—A frost, which came on in the night; all the ground hard; the sky was obscure with haze, and at the same time gentle wind. Showers of snow and rain in the afternoon and night. Stackencloud and wanecloud.

March 15.—Obscure morning and colder; some light snow showers and a clear windy night.

March 16.—Fine clear morning, but showers of snow and rain came in the afternoon.

March 17.—Clear morning. Wanecloud and other modifications were followed about noon by showers of snow and rain from the northward. Travelling on to Stirling, I found the atmosphere become much warmer. The sun came out, and the evening was mild and calm. The snow lay on the mountains while the lower lands were warm. I noticed stackenclouds or *cumuli* rising from behind the higher hills; others seemed to sit upon them, and to preserve the cumulativeness of structure just as when they float upon the diurnal vapour plane.

There seems to be the same almost constant tendency to produce rainclouds in these hilly regions hereabout, as I have before noticed in those of Wales. I have noticed also some other circumstances common to the atmospheric phænomena of both countries, and which probably belong to mountainous districts in general. The rain does not generally fall in such large and deluging streams as often happens in the flat countries of England. The interchange of sunny clearness and of showers is more rapid, and the sondercloud[*] and wanecloud[*] have not usually such a well defined character. This latter circumstance I cannot easily account for, except it be that the moister air of these regions conducts off more readily the electric fluid from the modifications, and thus weakens the specific cause of their particular characters. It can hardly be owing to the elevated peaks of the mountains approaching nearer to the clouds, as the wanecloud sometimes preserves its form when lodging, or at least in close apposition to the top of a mountain, in the same manner as it does to a mountainous stackencloud, as noticed by Aratus:

> *Ἡ νεφελη ὀρεως μυχησεται εν κορυφησιν,* &c.

The constant windy state of Edinburgh have I often noticed on the mountainous shores of England and Wales; and in a number of small balloons which were flying about over Edinburgh, I did not notice any to get a second current, as was generally the case in my experiments with balloons in England. I shall proceed, with your permission, to communicate further observations on the nephelogy of this country [illegible] your next.

I remain, sir, &c.

Stirling, March 17, 1816. THOMAS FORSTER.

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[*] i. e. *cirrocumulus* and *cirrostratus.*

METEOROLOGICAL TABLE

Extracted from the Register kept at Kinfauns Castle, N. Britain, the reisdence of Lord GRAY, Supposed Lat. 56° 23½′.—Above the Sea 129 feet.

1815.	Morning 8 o'clock. *Mean height of* Barom.	Morning 8 o'clock. *Mean height of* Ther.	Evening, 10 o'clock. *Mean height of* Barom.	Evening, 10 o'clock. *Mean height of* Ther.	Depth of Rain. Inch. 100	Nº of Days. Rain or Snow.	Nº of Days. Fair.
January.	29·74	32·58	29·73	31·87	1·00	9	22
February.	29·47	40·00	29·49	40·07	1·50	15	13
March.	29·36	39·96	29·38	39·61	1·50	18	13
April.	29·77	43·40	29·79	43·13	1·04	5	25
May.	29·68	51·54	29·70	49·64	2·22	15	16
June.	29·68	55·13	29·68	53·43	0 71	8	22
July.	29·85	57·61	29·85	55·61*	1·80	11	20
August.	29·62	56·83	29·61	55·51	1·52	8	23
September.	29·70	52·60	29·69	53·00	1·69	13	17
October.	29·62	47 41	29·59	47·90	2·42	15	16
November.	29·79	36·60	29·82	37·23	1·25	6	24
December.	29·55	33·00	29·56	33·00	1·35	9	22
Average of the year.	29·6525	45·555	29·6575	45·00	18·00	132	233

ANNUAL RESULTS.

MORNING.

Barometer. *Observations.*	*Wind.*		*Thermometer.*	*Wind.*	
Highest, 26th Nov.	SE.	30·55	13th July,	SE.	65°
Lowest, 13th Mar.	W.	28·55	17th Dec.	W.	17°

EVENING.

Highest, 25th Nov.	NW.	30 55	12th July,	SW.	63°
Lowest, 28th Dec.	NW.	28·60	19th Dec.	NW.	14°

Weather.	*Days.*
Fair	233
Rain or Snow	132
	365

Wind.	*Times*
N. and NE.	9
E. and SE.	102
S. and SW.	85
W. and NW.	169
	365

Extreme Cold and Heat, by Six's Thermometer.

Coldest, 23d December	Wind South	12°
Hottest, 29th June	SW.	73°
Mean of the year 1815		46·465

RESULT OF THREE RAIN GAGES.

	In. 100
No. 1. on a conical detached hill above the level of the Sea 600 feet	45 70
— 2. Centre of Garden, 20 feet	24·20
— 3. Kinfauns Castle, 129 feet	18·00
Mean of the 3 Gag	29·00

METEOR

METEOROLOGICAL JOURNAL KEPT AT BOSTON, LINCOLNSHIRE.

[The time of observation, unless otherwise, stated, is at 1 P.M.]

	Age of the Moon	Thermometer.	Barometer.	State of the Weather.	Modification of the Clouds.
1816.					
January 1	2	43·0	30·5	fair	cirro-cumulus
2	3			do	do
3	4			do	do
4	5	34·5	30·52	very fine	clear sky
5	6	44·0	30·30	cloudy and hazy	
6	7	46·0	29·20		cirro-cumulus, stratus, nimbus
7	8	42·5	30·02	windy	cirrus
8	9	48·5	29·61	fair	cloudy, cumulus
9	10	47·5	29·65	do	cumulus
10	11	43·0	29·60	do	stratus and cirrus
11	12	46·0	29·03	squally	cumulus widely spread
12	13	41·0	29·55	fair	cumulo-stratus
13	14	38·0	29·05	do	clear sky
14	15			rainy	
15	16	41·0	29·37	do	
16	17	41·5	29·79	fair	cumulus
17	18	37·0	29·54	do	cirrus
18	19				
19	20				
20	21	37·0	29·63	fair	cirro-cumulus in the horizon
21	22			rain	
22	23	43·0	29·75	fair	cumulus
23	24	41·0	29·55	do	do
24	25	38·5	29·44	do	do
25	26			rainy	
26	27	42·0	29·40	do	
27	28	38·5	29·85	do. & snow	
28	29			fair	
29	new	32·0	30·50	very fine	
30	1	30·5	30·55	do	
31	2	30·0	30·45	do	cirrus
February 1	3	28·0	30·09	do	do
2	4	29·0	29·75	fair	
3	5	40·5	29·55	very fine	do
4	6			fair	
5	7	38·0	29·50	very fine	

TABLE *continued.*

		Age of the Moon	Thermometer.	Barometer.	State of the Weather.	Modification of the Clouds.
February	6	8	36 5	29·35	rain & snow	
	7	9	31 0	29·20	snow	
	8	10	22 5	29 55	do	
	9	11	8.0 10 A.M 18 0 Noon 15 0 5 P.M 13 0 ¼ p 7 ditto	29 80	very fine	
	10	12	17 0 ¾ p.8 A.M 21 0 10 A.M 28 0 4 P.M.	29 80	fair	
	11	13	32 0	30 0	very fine	cirrus
	12	14	27 0	30 10	do	do
	13	full	33 5	30·10	cloudy	
	14	16	33 5	30 45	very fine	
	15	17	28 0	30 45	cloudy	
	16	18	43 0	29·95	do	a remarkable high tide [at 8 P.M.
	17	19	35 0	30·0	fair	
	18	20	33 5	30 0	cloudy	
	19	21	42 0	30·05	do	
	20	22	49 0	30·05	do	
	21	23	45 0	30 20	very fine	cirrus
	22	24	49 0	30·20	cloudy	
	23	25	46·0	30·35	do	
	24	26	43 0	30 25	very fine	
	25	27			cloudy	little rain in the evening
	26	28	41·0	30·25	very fine	
	27	29	41·0	29 75	rain & snow	
	28	new	36 5	29 95	fair	cirrus
	29	1	33·5	30 0	very fine	
March	1	2	35 0	30·0	do	
	2	3	41 0	29 60	cloudy	
	3	4			do	
	4	5	38 0	29 30	rain & snow	
	5	6	41 0	29 30	very fine	
	6	7	43 0	29 20	rain	
	7	8	42 0	[illegible]	very fine	
	8	9	42 0	29 20	cloudy	
	9	10	39 0	29 55	do	
	10	11			cloudy	
	11	12	50 0	29·85	do	
	12	13	51 0	29 60	rain	
	13	full	45·0	29·85	do	
	14	15	46 0	29·90	do	

METEORO-

METEOROLOGICAL TABLE,

BY MR. CARY, OF THE STRAND,

For March 1816.

Days of Month.		Thermometer. 8 o'Clock, Morning.	Noon.	11 o'Clock Night.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
Feb.	26	37	42	36	30·01	7	Cloudy
	27	36	49	40	29·60	0	Rain
	28	36	40	28	·80	27	Fair
	29	27	37	28	·90	20	Fair
March	1	28	42	40	.90	26	Fair
	2	36	43	40	·30	0	Rain
	3	38	41	38	·23	10	Cloudy
	4	37	47	32	·18	0	Showers with snow
	5	32	46	40	·20	14	Fair
	6	38	44	40	28·98	0	Rain
	7	40	45	39	·99	0	Rain
	8	39	42	36	29·00	0	Rain
	9	36	40	33	·45	0	Snow
	10	33	41	32	·73	12	Fair
	11	40	47	47	·80	0	Rain
	12	47	51	50	·72	0	Rain
	13	46	54	47	·70	14	Showery
	14	41	56	45	·50	15	Stormy
	15	47	54	30	·40	15	Stormy
	16	37	46	44	·80	22	Fair
	17	37	44	40	·50	0	Rain
	18	42	50	41	·48	10	Showery
	19	43	45	40	·78	26	Fair
	20	40	46	38	·90	29	Fair
	21	42	47	39	30·06	27	Fair
	22	43	48	40	·10	25	Fair
	23	41	46	38	·20	27	Fair
	24	38	38	37	·22	16	Cloudy
	25	38	42	40	·02	15	Cloudy
	26	40	42	38	·12	17	Cloudy

N.B. The Barometer's height is taken at one o'clock.

LI. *On the Cosmogony of Moses.* *By Mr.* ANDREW HORN.

To Mr. Tilloch.

SIR, — THE researches of geology have at length obtained for the cosmogony of Moses that attention which its intrinsic merit always demanded. The pages of the Philosophical Magazine have for several months past been occupied by various disquisitions upon the subject, particularly respecting the meaning of the term *day*. But in none of them, I conceive, has the true sense been elicited, as designating each of the six successive periods of the Genesis. Dr. Prichard has indeed pointed out the tropical sense in the term *period;* but its particular application in the cosmogony presents a difficulty which the figurative construction cannot by any means remove. Your correspondent F. E----s has taken advantage of this, and stated his objections in language sufficiently explicit to show his estimate of the Genesis and its author.

After being long engaged in a work illustrative of the Mosaic Cosmogony, which is nearly finished, allow me to say, that the results I have obtained from my investigations differ materially from those of all your correspondents upon this subject. However, as I agree with Dr. Prichard in his estimation of the Genesis, before I offer any opinion upon the import of the term *day*, as there used, I would observe, that although 'he does not' "place the author of the Pentateuch in the rank of common compilers of historical fragments, possessed merely of natural intelligence," he can have little difficulty in escaping from the dilemma, to which F. E——s imagines he has reduced him, in page 186 of your last number. If certain events recorded in the Genesis are found to agree with what may be called universal tradition, and some of those events never could have been conceived in any human mind without supernatural intelligence, this universal coincidence must, therefore, be referred to some common source. But we are not to suppose Moses the first person to whom the communication was made. Obvious reasons might be urged, why this favour should have been granted to the great progenitor of mankind. The notion of a *beginning*—that the universe once had *no existence*, most assuredly, is neither a dictate of reason nor a physical discovery. Now there never was a fact, left to tradition, but what has been corrupted. Was it possible, then, that the traditionary account of the origin of the world should during a progress of 3000 years have escaped corruption? When the facts, therefore, came to be recorded, they must have been so corrupted, that nothing less than inspiration could have enabled Moses or any other man to separate truth from error, to supply what was lost, and duly arrange the whole.

I presume that it is scarcely possible to obtain a just conception of any detached part or term in the Genesis, without previously knowing the author's principles. And this cannot well be attained without some acquaintance with the original language. Those in general who have possessed this requisite, have either twisted his expressions to countenance some preconceived hypothesis, or, taking a superficial view of the cosmogony, pronounced the whole unphilosophical and vulgar. After mature investigation, I do not hesitate to say that the two grand principles of the cosmogony are *atoms* and a *fluid.* Before we can, therefore, ascertain the sense of the word יום (*day*), we ought first to establish the meaning of the preceding term אור, commonly translated *light.* From a collation of passages where this word and its cognate terms occur, the ruling idea is, evidently, that of *fluidity.* The divine *fiat* for the existence of the *Aur* ought, therefore, to be rendered "Let a fluid be;" light being only one of its properties. Hence the term *æther*, so famous in the cosmogonies, and so ill defined in the systematic philosophy of the Greeks, is to be traced to the את האור (*Ath-eaur*) of the Mosaic cosmogony.

It is a vulgar error to suppose the sun was not created until the *fourth* day. Moses does not say that God *created*, but that [illegible] *made*, formed, or completed the sun in this period. According to the order of the narrative, the atomic masses of the earth and celestial bodies were brought into existence before the *æther*.

Having premised these things, we are now prepared to explain a part of the Genesis which has not a little perplexed expositors, and will lead us to the true explanation of the term *day*, as there used. Our common version reads, ver. 6: "God divided the light from the darkness." But darkness is no real being; and it is an absurdity into which Moses never could have fallen, to represent the Creator dividing things necessarily distinct. Now the difficulty is at once removed, by only supplying the word *earth*, the antecedent with which the passage evidently stands connected; thus literally, "And God divided (*the earth*) *between* the light and *between* the darkness." Hence the spheroidity of the earth is not only here intimated; but, as a globe cannot possibly have one hemisphere *enlightened* and the other *dark* but by light proceeding from an opposite focus, we have also a direct proof that the sun existed at this period, and operated upon the earth by transmitting its *fluid or light* feebly indeed at first, for it did not produce its full effect till the *fourth* day, when the Creator pronounced it "good," or fit "to regulate times and seasons:" so that from thenceforward they might be exactly calculated by its regular influence upon the motions of the earth.

Time is measured by the motion of bodies; and their velocity is quicker or slower according to the diameter of the rotatory body, and the quantity of force acting upon it. The planet Jupiter takes only about ten hours to perform one revolution upon its axis, which comprises a morning and an evening, and astronomers call this a day. The moon, again, employs about 1600 hours in making one rotation upon her axis, comprising also a morning and an evening, and this period is denominated a day. Hence also, in the phraseology of Moses, the term *day*, in each of the successive periods of the Genesis, properly denotes *one* rotation of the earth upon its axis, without any regard to the length or duration of the time; for, as the motions of our earth have been from the first moment dependent upon the sun, its influence upon the earth was at first extremely weak. Hence the diurnal rotation of the earth and progress in its orbit were then inconceivably slow; but the velocity of both motions gradually increased till the end of the fourth day, when the sun was perfected. The space of time, therefore, in each of the four first *days* or revolutions of the earth was of *indefinite* length, and each had "an *evening* and a *morning*." Hence these revolutions were true *solar days*: but every period or day differed from another; because the rotatory velocity of the earth was continually accelerated from the first moment till the end of the *fourth* day. The quantity of time therefore, or duration of any one preceding minute, or hour, was greater than any that succeeded; so that the first minute of the first day may have been equal, in duration or length of time, to a month or a year, compared with the last minute of the fourth day, the rotatory velocity in this minute being so much quicker. Thus it is evident that the *four* first days or rotations of the earth were periods that differed in their length, and their duration is *indefinite*.

I am, sir,

Your very obedient servant,

Wycombe, April 9, 1816. ANDREW HORN.

LII. *Brief Remarks on some indigenous Roses.* By M. J. WINCH, *Esq. of Newcastle.*

To Mr. Tilloch.

SIR,—THOUGH of late years the study of botany appears to have given way in a great measure to the more novel pursuits of mineralogy and geology; yet I trust that a few short observations on several Roses, natives of the north of England, may still be acceptable to a few of your readers, particularly as numerous doubts and difficulties are entertained by the most skilful botanists of the age respecting many species or varieties of this elegant and interesting but intricate genus.

Sir,
Your obedient servant,
Newcastle, March 24, 1816. M. J. Winch.

No. 1. *Rosa spinosissima.* Willd. Sp. Pl. 2. 1067. Fl. Brit. 2. 537. Eng. Bot. 187.
R. pimpinellifolia. Syst. Nat. ed. 10. 1062.
Flower-stalks smooth, fruit black.
On the sands of the sea-shore and in the alpine valleys of Teesdale.
The Burnet-leaved rose is a shrub of very humble growth, but rises to a tall bush in hedges and woods near this place.—The figure in English Botany is well delineated; but the fruit should have been almost black, to distinguish it more clearly from the next species,

No. 2. *Rosa rubella.* Eng. Bot. 2521.
R. spinosissima, var. 3. With. vol. 2. p. 465.—var. 8. Martyn's Miller's Gardener's Dictionary.
R. pimpinellifolia. Hort. Cantab. ed. 7. p. 154.?
Flower-stalks bristly, fruit scarlet.
This pretty rose, which is certainly very distinct from the preceding, occurs sparingly mixed with it on the sea-beach near Shields Law in the county of Durham. It appears to have been first noticed by a Mr. Atkinson, and transmitted to the late Dr. Withering from Landscall Haws in Lancashire, where several acres of land are covered by this highly ornamental shrub. Is not this called *R. pimpinellifolia* in some botanic gardens, and by the nursery-men in the vicinity of London?

No. 3. *Rosa involuta.* Fl. Brit. 3. 1398. Eng. Bot. 2068.
Fruit and flower-stalks very prickly.
Resembles *R. spinosissima* in its manner of growth; and in the shade rises to a tall shrub.—In Heaton Dean below Benton Bridge, Northumberland. Rare.

No. 4. *Rosa arvensis.* Willd. Sp. Pl. 2. 1066. Fl. Brit. 2. 538. Eng. Bot. 188.
In hedges and woods east of Newcastle frequent, generally bearing its flowers single (see var. 2. Hudson and Withering),

Withering), not clustered as represented in English Botany.

No. 5. *Rosa villosa.* Willd. Sp. Pl. 2. 1069. Fl. Brit. 2. 538. Eng. Bot. 583.

Sowerby's drawing is a good representation of the flower of this rose, as it may be observed in the rich country about Darlington. Near Newcastle the shrub becomes less luxuriant, and the petals are of a deeper hue.

No. 6. *Rosa mollis.* Eng. Bot. 2459.

R. villosa. β. mollissima. Willd. Sp. Pl. 2. 1070. Fl. Brit. 2. 538.

Rosa sylvestris folio molliter hirsuto, fructu rotundo glabro, calyce et pediculo hispidis. Dall in Dill. Raii Syn. 478.

When *R. villosa* grows on sterile soil or in a bleak situation, it assumes the stunted habit and full red flower of the specimen figured by Sowerby, and mentioned by Dillenius. This I consider as nothing but a variety of the preceding species. Near Newcastle it is extremely common, its fruit varying from perfect smoothness to a considerable degree of roughness, and the bush altering in mode of growth according to soil and exposure.

No. 7. *Rosa tomentosa.* Fl. Brit. 2. 539. Eng. Bot. 990.

This rose, which is by no means rare in the north, and forms one of the chief ornaments of the woods and hedges south-west from Newcastle, appears to be ill-understood by the Swedish botanists. The figure in English Botany represents the plant as flourishing in the south of England, and even about Darlington; but surely the prickles are too much hooked.—With us the shrub is less robust, its fruit smaller, and petals of a darker red.

No. 8. *Rosa rubiginosa.* Willd. Sp. Pl. 2. 1073. Fl. Brit. 2. 540. Eng. Bot. 991.

Though the sweet-briar may occasionally be met with on our ballast hills or in hedges, I suspect it is not indigenous in these places, but has been imported from the south of England in one instance, and carried from gardens by birds in the other,—the chalk-hills of Surrey, Kent, &c. appearing to be the original habitat of the Eglantine.

No. 9. *Rosa scabriuscula.* Eng. Bot. 1396.

Rosa sp. nov. Winch. Guide, vol. 1. 48. vol. 2. pref. 5.

Calyx permanent; fruit globose, bristly.

Var. *β.* Fruit smooth.

In woods and hedges about Newcastle frequent. The buds of this species are peculiarly handsome when sufficiently expanded to show the bright-red tints with which the outer edge of the white petals is marked.

No. 10. *Rosa canina.* Willd. Sp. Pl. 2. 1077. Fl. Brit. 2. 540. Eng. Bot. 992.

In hedges common.

No. 11. *Rosa glaucophylla.* Species Nova.

Calyx permanent; fruit ovate, smooth. Leaflets ovate pointed, doubly serrated, glaucous; prickles hooked.

This is a much slenderer, though less trailing briar than the foregoing: its flowers are pale pink, generally in pairs or single, and its fruit large. From *R. canina* it further differs in habit by not having young shoots sprouting beyond the blossoms, so as to give them the appearance of being axillary. To a rose named *R. senticosa* by Acharius in the Stockholm Transactions it seems more nearly allied than even to *R. canina;* but differs in the fruit being ovate, not globular, and the segments of its calyx less divided. This was sent me by Dr. Swartz, with the following remark: "R. canina *proxima, sed fructus subrotundus et aculei rectiores.*" I make no doubt the former has been often overlooked as a variety of the Dog Rose; it also resembles *R. cæsia* of English Botany, t. 2367, but differs in many points, besides having smooth, not downy leaves. By Mr. Woods, a gentleman who has paid particular attention to this genus, I am likewise assured they are distinct.

No. 12. *Rosa dumetorum.* Eng. Bot. 2579.

In a hedge on Friars-goose Quay below Gateshead, probably brought there with ballast from the south of England. This rose is accurately delineated in English Botany, but the calyx is long permanent on the fruit. No doubt can exist of its being different from every other British species. Like *R. canina* it frequently throws out long leading shoots which soon overtop the bunches of flowers.

No. 13. *Rosa alba.* Willd. Sp. Pl. 2. 1080.

Naturalized in a wood by the Tyne, above Hebburn Quay, Durham.

LIII. *On Dr.* MURRAY's *Opposition to Professor* PREVOST's *Theory of Radiant Caloric; on Electrical Phænomena, and on Sir* H. DAVY's *Safe Lamp.*

To Mr. Tilloch.

SIR,—DR. MURRAY has opposed the theory of Prevost on Radiant Heat. He (Dr. M.) says that the *polished metallic* surface of the canister containing a freezing mixture, when placed in the focus of a mirror, opposing another holding a thermometer, should *depress* the temperature *more* than the *blackened* side of the canister, if this theory be true; but that the contrary is the fact.

It is accepted that a blackened surface radiates heat more copiously than when the metallic surface is unimpaired; and it is inferred by the Doctor from this circumstance, that heat radiating in greater quantity from the canister by means of the black surface, should compensate more largely than the uncovered surface for the loss of temperature sustained by the thermometer in the opposing reflector. Experiment, however, teaches that the thermometer is less depressed by the clean metallic than the painted surface, *i. e.* there is a greater decrement of temperature by the dark surface than by that which has an unclouded lustre.

It must however be admitted, that the various colours *absorb* heat differently; black surfaces exceeding them in this respect. Hence white unpolished surfaces absorb heat slower than those that are blackened. The decrement of temperature may be accounted for by presuming that the facilities of absorption are in a more exalted ratio than the powers of radiation.

Mr. WALKER *on Electrical Phænomena.*

Your indefatigable correspondent Mr. W., among his quotations, refers to some experiments which do not tend to support the views of which he is the advocate. When aqueous vapour, it is stated, passes through an ignited porcelain tube (lined with charcoal), *hydrogen* is formed. This *is not the case*—A mixture of carbonic acid and hydrogen, as might be naturally expected, passes over into the recipient, forming *hydro-carbonate.* It is difficult to believe that pure *carbonic acid gas* can be obtained by passing steam through a tube lined with carbonate barytes. But grant it true, I can conceive that the quantum of heat employed may serve to detach *carbonic acid gas* from the carbonate barytes, and that this aëriform fluid may be found in the inverted vessel on the pneumatic cistern—no doubt in company with aqueous vapour—That carbonic acid gas is formed in *unlimited* quantity from a *limited* portion of carbonate barytes is *incredible.* Mr. Walker might as well adduce the experiment as a proof that water was partly if not wholly carbonic acid gas. The fixed air being displaced from the barytes, the water will unite with the earth and form a *hydrate.* In introducing the experiment of Sir H. Davy, your correspondent seems not aware of that of Dobereiner of Jena. The former distinguished chemist in electrifying *too great* a portion of mercury did not succeed in amalgamating it: but it is of a piece with that of Dobereiner. The latter, by using a minute portion of mercury in contact with water, and submitting it to the action of Voltaic electricity, found oxygen developed at the positive wire, while *no hydrogen* evolved at the negative extremity: but the fluidity of the mercury was arrested, and a solid amalgam formed; and this amalgam when submitted to heat gave off *hydrogen*, while the mercury resumed its *fluid form*. This would look as if hydrogen was a metal dissolved in caloric, and illustrates the electrization of ammonia associated with mercury—which phænomenon admits of an easy solution, if we admit nitrogen to be a compound of oxygen and hydrogen, of which I see no cause to doubt. I am not prepared to say that amalgamation with mercury is conclusive on the question of metallization—the experiments of Crosse would lead us to doubt it.

Sir HUMPHRY DAVY'S *Safe-lamp.*

I am happy in adding my complete conviction of the perfect safety of this invaluable discovery. It cannot be over-rated. The triumphs of humanity in this instance are complete, and will laud in grateful reminiscence the name of Davy.

On Thursday (28th ultimo) I descended the "William" Pit here, in company with that enlightened practical miner Mr. Peele, when Sir H. Davy's safe-lamp was put to the severest proof, in a recess of the most dangerous workings of the mine, and where a candle would have proved certain destruction. We only wanted the presence of Sir Humphry to have shared in our admiration of the imposing spectacle. The flame of the lamp was first capt with a blueish flame, a lambent light was then seen playing in the cylinder, and the flame of the fire-damp afterwards enlarging filled the whole wire-gauze. For some time the flame of the wick could be discerned within the lucid atmosphere of fire-damp, and then the whole was calmly extinguished. More brilliant results the friends of humanity could scarcely have dared to hope for. Sophistry is put to the blush. Independent of experiments which I had myself made with the safe-lamp, I placed implicit reliance on the manipulations of this illustrious chemist;—my confidence has not been shaken, nor have my expectations been deceived; and my conviction is now absolute.

As Sir H. Davy had recommended that the wire cylinder should be oiled when not in use, to preserve from oxidation, it was of importance to ascertain whether, should the flame of the fire-damp from its intensity set fire to the oil on the internal surface, it might not, by possibility, communicate with that on the exterior. For this purpose I obscured with naphtha on both sides a central portion of wire-gauze containing 3,600 meshes to the square inch, and having ignited this on one side, I found it did not communicate with that of the other, nor to alcohol, ether, or sulphuret of carbon applied by means of a camel hair pencil. I find I can even burn alcohol on each side, separated only by the fine open curtain of wire-gauze, without the flames "mingling into one," and this is beautifully illustrated from different colours being imparted to flaming alcohol by nitrates, copper, and strontites. My experiments with inflammable gases led to similar results.

I could not, by means of the blowpipe, force flame through this plexus, even when *red hot*, to fire ether applied by a pencil to the opposite side. I need not insist on the varied experiments I instituted. The application of wire-gauze to cut off the communication of flame is as extensive as the necessities and conveniences of life. Enveloped in a mantle of wire-gauze lined with woollen, we may run the gauntlet of flame, and defy its power.

Imprisoned in the cylinder of wire-gauze the captive fire-damp possesses no cause for alarm, exhibiting to the astonished eye an impressive and beautiful phænomenon.

I am respectfully, sir,
Very obediently yours,

Whitehaven, April 2, 1816. J. Murray.

LIV. *Description of the Menagerie at Schœnbrunn in Austria.*

By M. Marcel de Serres*.

The menagerie of Schœnbrunn is the most extensive in Europe. All the animals which it contains are separated from each other, and have a commodious asylum against the severity of the weather, and abundance of space for exercise. It is in the valleys of the fine park of Schœnbrunn that the dens or cages are to be seen for the various animals. The extremity of these rows of cages is merely closed with an iron gate, and the public have therefore a full view of all the animals in the menagerie. All of them have every thing they want beside them. Thus they have water in which to bathe, a thick shade to screen them from the sun, as well as an open field in which they may enjoy its rays.

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* *Annales des Arts et Manufactures*, tome lv. p. 248.

Several very rare animals have lived in the menagerie of Schœnbrunn: but we shall only instance at present the wild ox of Transylvania, known to naturalists by the name of *aurochs* (*Bos urus*), a species remarkable for the crest or mane it presents on its back. The *aurochs* is now an almost extinct breed, and it is only in Lithuania that it is to be met with: it should seem as if this species had been very abundant in the forests of that part of Europe, as well as in those of Hungary. Thus, as we have had occasion to observe in the living state the animal designated by Aristotle as the *bonasos*, and which the moderns had particularly distinguished by the name of *aurochs* and *bos urus*; we thought that the description of an animal so rare might be interesting to naturalists. We have therefore been induced to publish the following observations, which we had collected respecting an animal that naturalists have so seldom seen. This description might also have a high degree of interest, since the bones of the same individual which we have seen living, are deposited in the Natural History Museum at Paris.

The *aurochs*, or wild ox of the north of Europe, is a species known since the time of Aristotle, who alone of all the Greeks has left us a detailed description, in designating them under the name of *bonasos*. While thus designating the *aurochs*, the Pæonians called it *monapos*, and it seems that it was still known under the name of *monops*, of *monetos*, and of *monapos*. As, subsequently, various Latin authors have erroneously distinguished two kinds of wild oxen, some moderns, thinking they discovered the second species in the *bison*, have thought that the Greek expression *bonasos* was a translation of the Celto-Scythian word *won'nas*, formed of the article *as* and the Sclavonic word *wonny*, which signifies perfume. Thus, as Aristotle observed that the *bonasos* was called by the Pæonians *monapos*; this latter word has been derived from *monapux*, signifying having a thick mene. It is evident that all these etymologies may be well founded, since the *aurochs* exhales a very perceptible smell of musk, and presents a thick mane; but they are far from proving the opinion which is sought to be drawn.

The description given by Aristotle of the *aurochs* is too detailed to permit us to quote it entirely; but we shall give the principal heads of it: "The size of the *aurochs* is that of a bull, but shorter and thicker than the ox. Its skin is so large that it may serve as a bed for seven persons. Its mane is covered with softer and longer hairs than that of the horse. The breast of the *bonasos* is fawn-coloured, and its mane is of a reddish gray, and falls over the eyes and shoulders. The hair of the lower part or belly is darker, and like wool." Its feet are cloven, and the teeth as well as the interior parts are similar to those of the ox. The arrangement which Aristotle gives to the horns of the *aurochs*—of being folded and bent over each other in such a way that they cannot strike—is a peculiarity of the individual which Aristotle had before him; and which must have been correct, for he repeats the description twice. We know however that this character can scarcely be regarded as constant, since it varies in the same individual. Lastly, he assigns to each horn the size of a palm or upwards.

With the exception of the fable which Aristotle relates on the subject of the excrements of the *aurochs*, and which Pliny and Ælian have carefully preserved, the description of this great naturalist is distinct and clear. Finally, he assigns Pæonia as the country of the *bonasos*, and says that it inhabits Mount Menapius, which separates Pæonia from Media. Pliny, in afterwards copying the description of Aristotle, does not endeavour to ascertain what animal could be the *bonasos* of the Greek naturalist; he merely contents himself with saying: "Tradunt in Pæonia feram, quæ *bonasus* vocatur, equinâ jubâ, cætera *tauro* similem, cornibus ita in se flexis ut non sint utilia pugnæ; quapropter fugâ sibi auxiliari reddentem in eâ fimum interdum et trium jugerum longitudine: cujus contactus sequentes ut ignis aliquis amburat." Hitherto Pliny only speaks of one species of wild ox, as well as Aristotle; but in other places he designates two, since he says: "Jubatos *bisontes* excellentique vi et velocitate uros quibus imperitum vulgus bubalorum nomen imponit." He also returns to those two species of oxen; and observes that the Greeks had not experienced the medical properties of the wild oxen or *bisons*, with which the forests of India are filled. *Plin. Hist. Nat.* c. 28.

Solinus in copying the Latin naturalist also distinguishes two kinds of wild oxen; and he asserts that in the forests of Hyrcania the *bisons* are very common as well as the *urus*, and that these are the oxen distinguished by the vulgar under the name of *bubalus*. Oppian speaks of the *bonasos* in a manner as inexact as Pausanias, who, in describing the bulls exhibited in the spectacles at Rome, does not give the name of *bonasos*, but rather *bison*, making it come from Pæonia, the country of the former, as Aristotle had observed.

Cæsar, always accurate in his details, has decribed the *bonasus* of Aristotle by the name of *urus*. But he has described only one species of wild bull which he had observed in the forests of Hyrcania. This animal, he says, is very large, having the colour,

lour, stature, and carriage of a bull: their strength and agility are also, as he says, extreme[*].

Virgil in his Georgics, like Julius Cæsar in his Commentaries, makes mention of only one species of wild ox, which he also calls *urus*.

In the second book of his Georgics we find the following passage:

> "Texendæ sepes etiam, et pecus omne tenendum est,
> Præcipuè dum frons tenera imprudensque laborum;
> Cui, super indignas hyemes, solemque potentem,
> Sylvestres uri assiduè, capreæque sequaces
> Illudunt."
>
> *Geor.* lib. ii. v. 371.

In his third book he returns to the subject:

> "Tempore non alio dicunt regionibus illis
> Quæsitas ad sacra boves Junonis, et uris
> Imparibus ductos alta ad donaria currus."
>
> *Geor.* lib. iii. ver. 531.

Seneca and Martial distinguished, like Pliny, two species of wild oxen:

> "Tibi dant variæ pectora tigres,
> Tibi villosi terga *bisontes*,
> Latisque feri cornibus *uri*."
>
> SENEC. Hyppol.

> "Illi cessit atrox *bubalus* atque *bison*."
>
> MARTIAL.

As Pliny had already designated the *bonasos* of Aristotle by the name of *bubalus*, it is evident that Martial had Pliny in his eye.

Thus the ancients, with the exception of Cæsar and Aristotle, distinguished two kinds of wild oxen, which the moderns have endeavoured to trace: but the inquiry has led most of our modern naturalists to think that the oxen with humps on the shoulders were of the same race with the *aurochs*, and that all oxen with or without humps came from the wild ox of Lithuania. Gessner was the first modern naturalist who distinguished the *bison* of the ancients from the *urus*; and his opinion in addition to Buffon's has rendered the difficulties on the subject more intricate. It is important, in short, to know if the *bisons* of the ancients were the same species with the *bonasos* of Aristotle, and if the oxen of the new continent, called improperly *bisons*, belong also to the same race. Lastly, if the domestic oxen came from the wild oxen of Pæonia and Lithuania, or rather from another primitive stock. The most illustrious naturalist of our days (Buffon) endeavours to throw light on these delicate questions, without doing so, however, in the most satisfactory manner. Messrs. Cuvier, Lacépède, and Geoffroy have together nearly set the matter at rest. It results from their observations:

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[*] Lib. vi. 26. The edition of Julius Cæsar, by Clarke, printed at London in 1712, gives a very large figure to the *aurochs*, but it is very inaccurate. The neck and fore legs are by far too short, and the anterior part is by far too broad.

1. That it appears very clear that the *bonasos* of Aristotle is the same animal with the *urus* of Julius Cæsar, and that the *bison* of the Romans is this very *bonasos*, to which the appellation of *bubalus* has also been given.

2. That the ox with the hunch of the new continent, named improperly *bison*, is a species of ox which has nothing in common with the *bonasos* of Aristotle, who does not seem to have known the ox with the hunch. Besides, if Aristotle had known an ox with a hunch, he would not have asserted in the same place where he speaks of the *bonasos*, that the camel was the only quadruped which had a hunch on the back.

3. That we ought to acknowledge the zebra as the source of our race of domestic oxen, rather than the *aurochs*, which differs from the former by characters which it is not usual for nature to vary.

4. That the name of *urus*, applied by Julius Cæsar to the animal just described, might be a name originally German, as Aldrovandus and Gessner have already imagined; and this opinion appears to us much more probable than that of Macrobius, who regarded the name of *urus* as a French word. In fact, the Germans still designate the *urus* by the name of *auer-ochs*, *wald-ochs*, and *ur-ochs*, which signifies properly wild ox, or ox of the forests. Those who are familiar with the German language know how easy it is to imagine that the varieties in pronunciation adopted in Germany might have changed the word *urus* into *aurochs*: the fact is, the former is merely a softer pronunciation of the word. Servius explains the word *urus* in a manner much more learned; and he derives it from the Greek word ὄρος, which signifies mountain.

Description of the Aurochs or Bos Urus.

The size of the *aurochs* does not much surpass that of our largest-sized domestic oxen: but its form is much thicker and squatter. This thickness is the more remarkable when we compare it with the anterior part of the body of the *urus*, which is much broader. This greater breadth is particularly remarkable in the legs. But their larger size seems to depend merely on the muscles; for in the skeletons of the various species we do not remark any very marked differences.

The shape or contours are rugged, and not soft or round like those of the domestic ox, and all its limbs strongly indicate in some measure its savage nature and great strength. The thick and woolly mane which covers all the anterior part of the body of the *aurochs*, and the thick beard which hangs below its chin, give it a wild air, which the intenseness of its stare and its sombre and grave countenance render still more striking.

The head of the *aurochs* is very broad, nay, even almost square. In its greatest breadth it measures eleven inches five lines, while its greatest length is 18 inches. If we compare the breadth of the head of our domestic oxen with that of the *aurochs*, we find that they are nearly as 3 to 4. The forehead of the *aurochs* is also broader than it is high, and it is full and swoln, but less so than that of the *buffalo*.

This last character is even particular enough to enable us easily to distinguish this species from that of our domestic oxen, which have their forehead flat and a little concave. This disposition of the forehead with that of the horns separates those two species: thus the horns of the *aurochs* are not placed on the same level with the upper line of the forehead, but rather below, so that they form with the line of the forehead an acute angle. The horns of the common ox are on the contrary more elevated than the line of the forehead, so that they form with it an almost obtuse angle. As to the plan of the occiput, it is quadrangular in the domestic ox, while it is semicircular in the *aurochs*.

The salient angle of the interorbitary arcade forms in the *aurochs* a marked prominence of two inches, which is very considerable. The anterior of the head of the *urus*, the part where the nostrils are placed, is singularly flat and square in this species. This flatness is not found in any other species, and seems to be a character peculiarly inherent to the latter. The nostrils of the *aurochs* have this particularity, that they form almost a perfect circle; whereas those of the *buffalo* and domestic ox are decidedly oval. As to the black skin of the nostrils, it is thick and solid, and forms a thick flap.

The head of the *aurochs* is very hairy, and it is particularly under the chin that it presents a thick and occasionally a long beard. The ears of this species of ox are short, hairy, and placed behind above the horns. As to the horns, as their direction is subject to vary, and as the same individual presents them sometimes in opposite directions, we shall not stop to describe them. We shall content ourselves with observing here, that this direction is never from front to back, as in the *buffalo* for instance; besides, in this species they are thick and broad, but short: this is not the case always in the races of our domestic oxen; for instance, in the varieties of the oxen of Hungary, and even of Romania, which have very long horns.

The *urus* has a broad but short neck, and we do not see that the skin ever forms numerous and pendent folds, as observed in the domestic ox, and in the greater number of the species of this genus. It is generally very hairy and covered with a thick mane, which always becomes thicker towards the lower part. It is this mane which has made this species be confounded by several naturalists with the *bison* of the new continent, which seems to have a mane still more abundant.

The form of the body of the *aurochs* is generally thicker than that of the domestic oxen; and its body is also much more hairy, particularly in the fore parts and on the back. The hairs are generally very long and very thick. Finally, the *aurochs* has fourteen pair of ribs, while we observe but thirteen in all the other species of oxen.

The legs of the *urus* are short and thick, particularly the fore legs: they are also covered with long and numerous hairs; the feet are cloven and hairy, the hoofs are short, but strong and thick: as to the fetlocks, they are long and placed above the hoofs. From the direction of these fetlocks they almost touch the ground: those of the fore feet are shorter and more square than those of the hind feet, which are longer, but also not so broad.

The tail of the *aurochs* scarcely reaches half down the thigh; but the hair which covers it touches the ground: it is tufted, and the hair is long and thick.

The breast of the *aurochs* which we saw, was of a reddish brown, or of a dull fawn colour, and almost of one tint: the hair of the body, as well as the mane which covered the whole of the neck and part of the shoulders, had not that grayish tinge which Aristotle makes one of the characters of the *bonasos.* The hair of the *urus* is not short and frizzled, but, on the contrary, long and straight, and in the individual just mentioned it is of a generally uniform colour.

The *aurochs* presents, therefore, as its chief characters: a face very flat, the forehead slightly swoln, the horns placed below the line of the occiput, and the interorbitary arcade very salient: Lastly, a thick mane and fourteen pair of ribs. The characters presented by the occiput, of being arranged in a semicircle, and of forming an obtuse angle with the forehead, are also equally important, and may very clearly serve for the distinction of this species.

The dimensions of the *aurochs* are as follow:

Length following the curvature of the back, nine feet three inches.

Length from the orbit to the anus, seven feet seven inches.

Height taken at the highest point of the back, five feet nine inches.

Length of the head, one foot ten inches.

Breadth, one foot four inches.

Length from the extremity of the nostrils to the extremity of the eye, one foot.

From the extremity of the eye to the root of the horns, five inches.

Breadth of the horns taken at their base, four inches.

Length of the neck to the shoulder-blade taken obliquely, one foot five inches.

Height from the hoof of the fore foot with the different curvatures to the most elevated part of the back, five feet nine inches.

Space between the train behind and the train before, two feet eight inches.

Height from the fore foot to the chest, two feet five inches.

Height from the fore feet to the line of the body, two feet two inches.

Height from the hind feet measured to the line of the body, two feet seven inches.

Length of the neck, one foot eight inches.

Length of the tail, four feet.

We may refer to the *aurochs* the descriptions and figures under mentioned.

1. The species which Aristotle has described by the name of *bonasos*.

2. The species of oxen described by the Greeks by the names of *monapos*, *monepos*, and *monops*.

3. The oxen described by Pliny, under the names of *bonasus*, *bubalus*, and *bison*, as well as all those which the ancient Latin authors called by these names. We must not confound with the *aurochs* the species which some moderns have described by the name of *bubalus*, which is nothing but the *buffalo*.

4. The *urus* of Julius Cæsar and Virgil.

5. The figures of Aldrovandus are so inaccurate that we can scarcely refer to the *aurochs* that which he gives of the *urus*. As to Johnston, as he has adopted the opinion of certain Latin authors in distinguishing the two species of wild oxen, he has also given drawings of them. In plate xix. he has given the *bonasos*; and in plate xx. the *urus*. If these two figures were accurate, they would certainly indicate two very different animals; but when we consider the little credit which Johnston deserves, we cannot refer to them with propriety.

6. The ox figured in the Commentaries of Sigismond D'Herberstein.

7. The ox represented by Anthony Wied in the Map of Muscovy.

8. The ox represented in the large edition of Julius Cæsar published at London by Clarke. In this figure the feet are represented by far too small to support the mass of the body.

9. The *zimbre* or wild ox of Moldavia, or the *zuber*, *zubr*, *tur* or *turon* of Poland, is certainly the *aurochs*.

10. The white *bison* of Forster and Pennant is only a variety of the *aurochs*.

11. The figure of the female urus, given by Gilibert in his History of Quadrupeds.

12. The figure of the *aurochs* of Bertuch, plate xiv. fig. 1, which is to be found in the *Tafeln der Allgemeinen Naturgeschichte*. The figures given by Bertuch are so small, and in general so bad, that we can place but little confidence in them.

13. The descriptions which Pallas has given of the *urus* are exact, and he also cites another description of the anatomist Wilde, which has the same advantage.

The *aurochs* which we saw was a male, which had lived thirty years in the menagerie. It came from Transylvania. In a fire which broke out in the place where it was kept, it owed its preservation to its great strength; for it broke the chains by which it was fastened, dug up the door of its den, and overthrowing every obstacle, escaped from the flames. This great strength of the *aurochs* would make it very valuable if it could be trained for domestic purposes; but all the attempts made to tame this one at Schœnbrunn were fruitless. When I saw it, it was not quite so ferocious, for age had diminished its strength. The branches of trees which had been given it for food, and of which it was very fond, had so worn down its teeth that it could no longer digest but with pain, not being able to masticate. It died soon afterwards of consumption.

The *aurochs* is a species almost lost in the present day: some few individuals only are found thinly scattered in the forests of Lithuania and Transylvania. We are assured that there are some in the Krapac Mountains also. It appears that it was formerly very common in some temperate regions of Europe. The descriptions of ancient Latin authors give us every reason to believe so.

The individual which we saw at Schœnbrunn emitted a hoarse but very strong sound, and which had something in it sad and mournful: it roared, but never bellowed like an ox. When it was frightened or crossed in any way, it roared so loud that its keepers were [illegible] for the consequences.

LV. *On the Cosmogony of Moses. By Dr.* Prichard; *in Reply to* F. E——s.

To Mr. Tilloch.

Sir, — In the last number of the Philosophical Magazine I observe a second attack upon my paper on the Mosaic Cosmogony. I had declined entering into any further controversy on this subject; but on mature consideration it appears to me a matter of so much importance to prove that the exordium of the Hebrew Scriptures is capable of a rational and philosophical interpretation, instead of one which gives [illegible] very much the air of a piece of mythology, that I have ventured to [illegible] to the strictures of your correspondent.

He has detected me in one error, to which I must certainly plead guilty; viz. that of citing Laplace's *Système du Monde* by a designation not quite accurate. I wrote in great haste, and had not leisure for correcting my composition; and I could even point out to my keen-sighted adversary some other verbal inaccuracies equally unimportant, which I discovered in looking over my paper since I received the number of the Philosophical Magazine which contains it; but as these matters have little relation to the argument before us, I shall proceed to consider on what grounds I am accused by Mr. F. E——s of contradicting my own statements.

I observed in my first paper, that the speculations of heathen philosophers on the Cosmogony were founded on some fanciful analogy with natural processes that are daily observed; and in my last communication I have said that the historical documents contained in the early part of Genesis may be traced among many remote nations; and that not only the Asiatics but the Scandinavians and Mexicans "were equally in possession of the primitive traditions." But I beg to have it observed, that in this latter instance I made no allusion to the Cosmogony. Neither the Scandinavians nor the Mexicans had, as far as I remember, any notions on this subject which bore the slightest analogy to the account which we have in the Pentateuch. The former on the contrary held that the earth was in the beginning the dead body of a huge giant, while the latter imputed the origin of the human species to the fall of an aërolite. Yet in the mythological fables of both these nations there are many circumstances referring to the history of mankind, which were evidently derived from the same source with some relations contained in the early Scriptures of the Hebrews; and it was only for the sake of proving this connexion, and the consequence which I deduced from it, that they were alluded to. I may add that those nations who preserved the tradition of the Cosmogony, the Hindoos for example, added so many fictitious circumstances to it, that in the character which it assumes in their hands it may well be contrasted with that simple narrative of events which we find in the beginning of Genesis.

Neither is the hypothesis, that the early parts of this book are a compilation—incompatible with the opinion that Moses was an inspired writer; unless it be proved that all inspired persons have possessed the attribute of omniscience, which I am not disposed to concede as a self-evident truth. If, as Mr. F. E——s maintains, the supposition that Moses had recourse to previously existing documents implies the want of inspiration, I would beg to ask whether St. Matthew and St. Luke were in want of inspiration when they had recourse to previously existing documents in compiling their genealogies? Or, if they wrote from immediate revelation, how happened it that one of them followed the Hebrew text, and the other the Septuagint translation? It is absurd to hold any argument on the extent of endowments concerning the nature and limitations of which we are wholly ignorant; but the example above cited is sufficient to prove that it is not beneath the dignity of an inspired writer to avail himself of historical documents where any such exist. Michaelis has fully proved that Moses framed his code of laws by combining the ancient usages of the nomadic Hebrews with the institutions of the agricultural Egyptians. Surely then we may venture to conjecture that he adopted into his annals the most authentic documents that existed concerning the history of the world. But the account of the Cosmogony must have been at first derived from a particular revelation, and therefore it is just as easy to suppose that it was revealed to Moses as to Enoch or Noah. I allow the force of this argument, and should acquiesce in the conclusion drawn from it, if there were not facts which appear to prove that the document in question is really more ancient than the age of Moses.

Notwithstanding Mr. F. E——s's objections, it is still manifest that the Cosmogony of Menu bears a remarkable analogy to that of Moses. In the former it is said that at the close or termination of each night a reiterated act of creation took place. How does this differ essentially from the force of the phrases "'The evening had come and the morning had come—one day—when God said, Let there be a firmament,' &c.? This is not a day included by its natural limits, otherwise it would have been [illegible] *the morning and evening*. The expressions are equivalent to these: After the *close* of one day, and when the *dawning* of another had appeared, the act of creation was renewed.' *Twilight* is the word that occurs in the translation of Menu. We are presently told by Menu that each day comprehended a long succession of ages. In the Etruscan Cosmogony, which bears a still more striking analogy to the Hebrew, and on which your correspondent has forgotten to make any remark, the duration of each period is expressly mentioned to have been a chiliad, or a thousand years. If the Hindoos and Etruscans derived these records from the same source whence Moses obtained his account, which I think almost certain, the conclusion I have drawn seems to be scarcely avoidable.

I now proceed to "*the other and more weighty objection—that it does not appear that the Hebrew people ever understood the six days of the creation*" in any other sense than the literal one. And here I would beg leave to ask Mr. F. E——s how we are to interpret the passage in which it is said that God created man after his own image. Are we to understand by a metaphor, that man was created an intellectual and moral being; or must we receive the expressions literally, as we have reason to believe the Hebrew people did, who are supposed on good grounds to have been anthropomorphites? If so, I presume we must also give a literal interpretation to such phrases as "*It repented God,*" "*God rested on the seventh day,*" &c. &c. And if we succeed in establishing all these points, we shall at length bring the theology of Moses much more nearly on a level with that of Hesiod than it has been supposed to be. If, however, as I suppose it will be conceded, these expressions demand a figurative explanation, though the Hebrew people, at least the vulgar, understood them literally; I am at liberty to assume the same latitude with respect to the *days* of the creation. While adverting to the prevalent notions of the Hebrews on this subject, it is somewhat surprising that Mr. F. E——s did not think it worth while to take notice of the opinions of the two most learned antiquaries of that nation, viz. Josephus and Philo, whom I cited in my last paper, and who expressly affirm that the account of the six days' work is metaphorical. Philo particularly says, "It is a *piece of rustic simplicity* to understand it literally."

Lastly; Mr. F. E——s says that I have effected the coincidences which I call upon him to admire between the Cosmogony and the Epochs of Nature, by transferring the creation of zoöphytes and testacea from the fifth day to the third; whereas, according to the sense in which the twentieth and two following verses in the first chapter of Genesis "*have hitherto been generally understood, all the inhabitants of the waters were called into existence on the fifth day.*" It is of little importance in what sense these passages have been generally understood by careless readers, if the expressions they contain will not bear the construction imputed to them. The LXX certainly did not understand the words which describe the fifth day's creation as including corals and bivalves, otherwise they surely would not have rendered them *τὰ κήτη τὰ μεγάλα, καὶ πᾶσαν ψυχὴν ζώων ἑρπετῶν*. The Hebrew word which the LXX have rendered *ἑρπετὰ* gives the idea of progressive motion; and it occurs in a similar sense in many other places, but particularly in Psalm civ. 20, where it is applied to the roving of wild beasts in the night. It is therefore very certain, as your correspondent has hinted, that Moses has assigned no place for zoöphytes and testacea; neither has he mentioned forest-trees, or shrubs, or lichens, for he did not write the Cosmogony with the *Systema Naturæ* before him. The Hebrew language being very poor in terms of classification, a few leading objects in each class are mentioned; and we are left to understand that the analogous kinds were conjoined with those which are named. Thus in the third day's creation we are told that "*grass and seed-bearing herbs and fruit-bearing trees*" began to exist; and we are left to supply all the remainder of the vegetable world, including marine plants to which no allusion is made. It is not going much further out of the way to add corals and madrepores, and even testacea, considering that no other place is allotted to them, and that on the next day whales and progressive aquatic animals only are mentioned. At any rate, it is evident that Moses had it not in contemplation to make a complete census of the whole number of created beings: and when we find zoöphytes and testacea associated in the earth with the earliest remains of vegetable productions, and evidently belonging to the same epoch in the creation, the presence of the former constitutes no exception to the coincidence which would otherwise be complete.

Mr. F. E——s seems to have given up his hypothesis for explaining the revolution of night and day before the creation of the sun, and even avows that he was aware of the physical circumstances which render it untenable, before he advanced it. There is one interpretation, however, of the fourteenth and following verses which will assist him out of this difficulty, and will even remove one great objection to the sense he attaches to the six days. The explanation I allude to, is that which was proposed by Dr. Geddes, a learned but bold commentator, whose method of criticism merits in many instances the severest reprehension. In this particular passage I believe he has discovered the true sense; and I shall therefore cite his translation of it, though it deprives me of one argument in favour of my own hypothesis which has not been answered. Dr. G. considers the words, "*Let there be lights in the firmament of heaven to illuminate the earth,*" as equivalent to "*Let lights appear,*" &c.

and he proves that the words here used do not necessarily imply a new creation of the sun, and cites the authority of Origen who was of the same opinion. Accordingly the sun may have existed before this period, though it only now became visible on the earth in consequence of the changes which had taken place in the terrestrial atmosphere. On this hypothesis all the motions of the solar system may be supposed to have subsisted for ages before the epoch to which the Mosaic Cosmogony (except perhaps the first verse of it) refers.

If this interpretation be allowed, the whole Cosmogony will present a scheme perfectly rational and consistent with itself, as well as coinciding admirably with the information which natural philosophers have gained concerning the system of the world. It is well known that certain stars have at various times disappeared from the heavens or have ceased to be luminous, while others have been observed which were not before visible; as the star observed by Hipparchus, which induced that celebrated astronomer to make a catalogue of the heavenly bodies, and that which shone for a time with so much splendor in Cassiopœia. Others have been for a time either partially or wholly obscured, and have again become luminous. Hence it is probable that those which have been observed for the first time, really existed in the heavens before they were seen, and remained for many ages in a dark unilluminated state. Such may have been the condition of our sun and solar system before the period when light is said by Moses to have appeared in the world; and during the preceding ages the state of the earth revolving round a dark sun is well described by the expressions "*The earth was desolate and void, and darkness upon the face of the deep.*" If these speculations are allowed, in which no cause is assumed that is not known really to exist, I would venture to propose the following paraphrase of the Cosmogony:

Ver. 1. At some very remote epoch God created the material universe.

[Through the remainder of the chapter Moses confines his attention to the changes that were effected in this terraqueous globe, and only "mentions such other parts of the universe as became eventually correlative to it."]

Ver. 2. The world remained long desolate and void, and a *dark mist*[*] environed the surface of our globe.

At length the Divine Energy (the Spirit of God) began to exert itself, and to set in action that train of physical changes which was destined to develop the organized creation.

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[*] This is the sense of the Hebrew word חשך.—See Parkhurst's and Buxtorf's Lexicons.

Ver. 3—4. The solar system became illuminated by the sun, which had hitherto been a dark body. "The light here mentioned," says Dr. Geddes, "may even from the context itself be readily supposed to be but an imperfect and partial light, such as we often see in a foggy day; which light would gradually increase in proportion as the air was expanded and rarefied, until on the fourth day it received the utmost degree of brightness from the unclouded appearance of the sun." This indeed seems to be the only sense in which it is possible to understand the existence of light before the appearance of the sun. The common interpretation of the "days" of the creation is thus rendered more tenable than it can otherwise become.

Ver. 5. Termination of the first period.

Ver. 6—7. The atmosphere becoming less dense, is called an *expanse*, in which the aërial waters or clouds now float in separate masses.

Ver. 8. Termination of the second period.

Ver. 9. The higher regions of the earth emerge from the universal ocean, never again to be covered by it until the era of the deluge.

Ver. 11—12. "*Grass and seed-bearing herbs and fruit-bearing trees created.*" Moses only alludes here to the vegetable creation on dry land. He makes no mention of marine plants, corals, madrepores, &c. or testacea.

Ver. 13. Termination of the third period.

Ver. 14, &c. The heavenly bodies now first shine clearly on the surface of the earth.

Ver. 19. Termination of the fourth period.

Ver. 20—21. Whales and fishes produced in the sea, and birds on the land.

Ver. 23. Termination of the fifth period.

Ver. 24, &c. Creation of land quadrupeds.

Ver. 25, &c. Creation of man.

Your correspondent will of course treat this interpretation with contempt, unless he should happen to think it favourable to his own hypothesis: but I have little doubt of its appearing to unprejudiced persons the most probable mode of explaining the cosmogony.

I have the honour to be,
Sir,
Your obedient servant,

Bristol, April 11, 1816. J. C. PRICHARD.

LVI. *Mr.* Hume *on Emetic Tartar.*

To Mr. Tilloch.

Sir, — In addition to what I have already communicated* to you, in regard to my methods of preparing *emetic tartar*, I beg to submit to your readers a copy of a letter which I sent, above two years ago, to the President of the Royal College of Physicians, London.

This I hope will serve as a protection against the officious and, I may say, illiberal, intrusion of one, who seems still determined to injure my professional reputation and, consequently, the welfare of my family.

I have said *methods*, because, by more ways than one, I have effected the decomposition, and have proved that an oxide of antimony, fit for the purpose, can be obtained directly from the common black sulphuret either by means of *nitric acid* alone, properly diluted with water, or the *ingredients* themselves by which this acid is prepared, together with the necessary quantity of water.

The process which I sent to you, and which you have already admitted into the Philosophical Magazine, has been supported by the most ample and favourable testimonies, both by manufacturers and others who are competent to judge of its merits; I have therefore no inducement at present to recommend any change in that prescription.

I take this occasion to declare that, excepting the preparation of the medicine in question, the tartarized antimony, I had less to do with the changes that have been made either in the former volume or in the *editio nova* of the Pharmacopœia than may be supposed; I had no direct communication with the committee; nor do I know, even at this moment, how and of whom this committee was composed, whether it was open to all the members of the College, or secret and select: consequently I had no opportunity of preventing the material error (*tum cola*) which had unfortunately slipped into the formula for *antimonium tartarizatum.*

The letter, of which I shall subjoin a copy, was never meant to be kept as a secret, rather than to be presented *first* to the consideration of the committee, which, as I understood, had been appointed to reform the Pharmacopœia of the London College. This letter, if admitted into your pages, will serve as a registry, notwithstanding all that has been asserted by one who has written so much and done so little, that the process which the College has now abandoned might have been profitably modified;

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* Philosophical Magazine, vol. xlv. p. 301.

that the nitric acid was not in such great excess had a sufficient quantity of *water* been employed, and that the muriatic acid is not of so much consequence in the formation of protoxide of antimony, since the whole of the eleven fluid ounces there prescribed might have been omitted.

I remain, sir,
Your obedient servant,
Long Acre, April 17, 1816. Jos. Hume.

"*To Dr. Latham, &c. &c. &c.*

"Sir,—A very simple and, I believe, effectual method of preparing tartarized antimony occurred to me yesterday, which I am anxious to present to you lest I should be anticipated unfairly by any other experimentalist.

"I shall not at present take up more of your time than to state the following sketch of the process.

"The common black sulphuret of antimony is boiled with nitrous acid *largely* diluted with water. This produces an oxide of antimony which, after being properly washed, is to be boiled with supertartrate of potass and distilled water. The operation is then to be finished in the usual manner by evaporation and crystallization.

"I have the honour to be, Sir, &c.
Long-Acre, Jan. 21, 1814. "Jos. Hume."

LVII. *On constructing Electric Columns.* *By* B. M. Forstel, *Esq.*

To Mr. Tilloch.

Sir,—I have lately formed an *electric column*, by a process somewhat different from any hitherto adopted, I believe; and imagining that it may prove a very convenient one, I wish by means of your publication to make it known, hoping that those persons who are interested about this curious instrument will make experiments to determine the comparative power of a *column* formed by this method, and one made according to Mr. De Luc's original plan.

Not being able easily to procure *manganese* in a finely-powdered state, which I understand has been made use of instead of *plates* of *zinc*, I was desirous of trying the effect of *zinc* very finely powdered; and having obtained some of this substance pulverized, in order to get the very fine particles I sifted it through muslin, then mixed it with a solution of common glue and some moist sugar. This mixture I laid on the back of *Dutch-*

Dutch-gilt embossed paper with a brush, by which method a triple disc was obtained, consisting of *Dutch metal* (copper), *paper* and *zinc*. Perhaps some one may be able to describe the exact method of making the *Dutch-gilt paper;* if so, I shall esteem it a favour to be informed of it. I understand that the metal is formed of copper principally, but of what other substance is unknown to me. It appears to me that a considerable advantage may be gained by the above-mentioned process, both in time and labour, as these plates or discs are much more easily punched out than plates of zinc, and the placing them in a column is also much more easily done on account of there being only one kind of disc, therefore less chance of a mistake is likely to occur in the disposition of them. If paper with *silver-leaf* laid on it, as described in Philosophical Magazine, vol. xxxv. p. 205 (March 1810), were used instead of *Dutch-gilt paper,* the effect would probably be stronger, and still more so if discs of *silvered paper* and discs of paper with powdered *zinc* were used, as the paper would then be more capable of imbibing moisture from the atmosphere than when both sides are coated with metal. As the gold-leaf electrometer, commonly used for experiments with the electric column, is rather an expensive one, and troublesome on account of the delicacy of the gold-leaves, it may not be unacceptable to many of your readers to be informed, that a metal-leaved electrometer of great sensibility may at a very reasonable cost be made, with a small lamp-glass fitted up with a *mahogany* top and base; the leaves are to be made of *Dutch metal* fastened to a thick piece of brass wire or slip of wood fixed into a piece of cork (which is to be glued on to the wooden cap); slips of *tin-foil* or *Dutch-metal* are to be pasted on two opposite parts of the glass cylinder as usual.

Electrometers made with *Dutch-metal* as above mentioned, are strongly affected by a person's finger being placed on them, if he suddenly rise from a chair (with a horse-hair seat) and stand upon a dry fire-hearth, the electric effects being produced by his woollen clothing rubbing against the horse-hair chair.

A dry fire-hearth being found to be *a very good* insulator, I frequently make use of it instead of a stool with glass legs. Should it be found that *zinc* dust laid on, as in the foregoing account, does answer the purpose desired; this method may be worthy of the attention of the philosophical-instrument-makers, as they might probably find a sale for this (as it may be called) *electrical paper.* If zinc could be procured in as thin leaves as silver or *Dutch-metal,* it probably would be better for *electric columns* than *zinc* dust.

April 17, 1816. **B. M. Forster.**

LVIII. *Re-*

LVIII. *Report of the Select Committee of the House of Commons, appointed to inquire, whether it be expedient that the Collection mentioned in the Earl of* ELGIN'S *Petition, presented to the House on the 15th Day of February last, should be purchased on behalf of the Public; and if so, what Price it may be reasonable to allow for the same.*

THE Select Committee consider the subject referred to them, as divided into four principal heads;

The first of which relates to the authority by which this collection was acquired:—The second to the circumstances under which that authority was granted:—The third to the merit of the marbles as works of sculpture, and the importance of making them public property, for the purpose of promoting the study of the fine arts in Great Britain;—and The fourth to their value as objects of sale; which includes the consideration of the expense which has attended the removing, transporting, and bringing them to England. To these will be added some general observations upon what is to be found, in various authors, relating to these marbles.

I. When the Earl of Elgin quitted England upon his mission to the Ottoman Porte, it was his original intention to make that appointment beneficial to the progress of the fine arts in Great Britain, by procuring accurate drawings and casts of the valuable remains of sculpture and architecture scattered throughout Greece, and particularly concentrated at Athens.

With this view he engaged Signor Lusieri, a painter of reputation, who was then in the service of the King of the Two Sicilies, together with two architects, two modellers, and a figure-painter, whom Mr. Hamilton (now under-secretary of state) engaged at Rome and dispatched with Lusieri, in the summer of 1800, from Constantinople to Athens.

They were employed there about nine months, from August 1800 to May 1801, without having any sort of facility or accommodation afforded to them: nor was the Acropolis accessible to them, even for the purpose of taking drawings, except by the payment of a large fee, which was exacted daily.

The other five artists were withdrawn from Athens in January 1803; but Lusieri has continued there ever since, excepting during the short period of our hostilities with the Ottoman Porte.

During the year 1800, Egypt was in the power of the French: and that sort of contempt and dislike which has always characterized the Turkish government and people in their behaviour towards every denomination of Christians, prevailed in full force.

The success of the British arms in Egypt, and the expected restitution of that province to the Porte, wrought a wonderful and instantaneous change in the disposition of all ranks and descriptions of people towards our nation. Universal benevolence and good-will appeared to take place of suspicion and aversion. Nothing was refused which was asked; and Lord Elgin, availing himself of this favourable and unexpected alteration, obtained, in the summer of 1801, access to the Acropolis for general purposes, with permission to draw, model, and remove; to which was added, a special license to excavate in a particular place. Lord Elgin mentions in his evidence, that he was obliged to send from Athens to Constantinople for leave to remove a house; at the same time remarking that, in point of fact, all permissions issuing from the Porte to any distant provinces, are little better than authorities to make the best bargain that can be made with the local magistracies. The applications upon this subject passed in verbal conversations: but the warrants or fermauns were granted in writing, addressed to the chief authorities resident at Athens, to whom they were delivered, and in whose hands they remained: so that your committee had no opportunity of learning from Lord Elgin himself their exact tenor, or of ascertaining in what terms they noticed, or allowed, the displacing or carrying away of these marbles. But Dr. Hunt, who accompanied Lord Elgin as chaplain to the embassy, has preserved, and has now in his possession, a translation of the second fermaun, which extended the powers of the first; but as he had it not with him in London, to produce before your committee, he stated the substance, according to his recollection, which was "That, in order to show their particular respect to the ambassador of Great Britain, the august ally of the Porte, with whom they were now and had long been in the strictest alliance, they gave to his excellency and to his secretary, and the artists employed by him, the most extensive permission to view, draw and model the ancient temples of the idols, and the sculptures upon them, and to make excavations, and to take away any stones that might appear interesting to them." He stated further, that no remonstrance was at any time made, nor any displeasure shown by the Turkish government, either at Constantinople or at Athens, against the extensive interpretation which was put upon this fermaun; and although the work of taking down and removing was going on for months, and even years, and was conducted in the most public manner, numbers of native labourers, to the amount of some hundreds, being frequently employed, not the least obstruction was ever interposed, nor the smallest uneasiness shown after the granting of this second fermaun. Among the Greek population and inhabitants of Athens it occasioned no sort of dissatisfaction; but, as Mr. Hamilton, an eye-witness, expresses it, so far from exciting any unpleasant sensation, the people seemed to feel it as the means of bringing foreigners into their country, and of having money spent among them. The Turks showed a total indifference and apathy as to the preservation of these remains, except when in a fit of wanton destruction they sometimes carried their disregard so far as to do mischief by firing at them. The numerous travellers and admirers of the arts committed greater waste, from a very different motive; for many of those who visited the Acropolis tempted the soldiers and other people about the fortress to bring them down heads, legs, or arms, or whatever other pieces they could carry off.

A translation of the fermaun itself has since been forwarded by Dr. Hunt, which is printed in the Appendix.

II. Upon the second division, it must be premised, that antecedently to Lord Elgin's departure for Constantinople. he communicated his intentions of bringing home casts and drawings from Athens, for the benefit and advancement of the fine arts in this country, to Mr. Pitt, Lord Grenville, and Mr. Dundas, suggesting to them the propriety of considering it as a national object, fit to be undertaken and carried into effect at the public expense; but that this recommendation was in no degree encouraged, either at that time or afterwards.

It is evident, from a letter of Lord Elgin to the secretary of state, 13 January, 1803, that he considered himself as having no sort of claim for his disbursements in the prosecution of these pursuits, though he stated, in the same dispatch, the heavy expenses in which they had involved him, so as to make it extremely inconvenient for him to forgo any of the usual allowances to which ambassadors at other courts were entitled. It cannot, therefore, be doubted, that he looked upon himself in this respect as acting in a character entirely distinct from his official situation. But whether the government from whom he obtained permission did, or could so consider him, is a question which can be solved only by conjecture and reasoning, in the absence and deficiency of all positive testimony. The Turkish ministers of that day are, in fact, the only persons in the world capable (if they are still alive) of deciding the doubt; and it is probable that even they, if it were possible to consult them, might be unable to form any very distinct discrimination as to the character in consideration of which they acceded to Lord Elgin's request. The occasion made them, beyond all precedent, propitious to whatever was desired in behalf of the English nation; they readily, therefore, complied with all that was asked by Lord Elgin. He was an Englishman of high rank; he was also ambassador from our court: they granted the same permission to no other individual: but then, as Lord Elgin observes, no other individual applied for it to the same extent, nor had indeed the same unlimited means for carrying such an undertaking into execution. The expression of one of the most intelligent and distinguished of the British travellers, who visited Athens about the same period, appears to your committee to convey as correct a judgement as can be formed upon this question, which is incapable of being satisfactorily separated, and must be taken in the aggregate.

The Earl of Aberdeen, in answer to an inquiry, whether the authority and influence of a public situation was in his opinion necessary for accomplishing the removal of these marbles, answered, that he did not think a private individual could have accomplished the removal of the remains which Lord Elgin obtained: and Dr. Hunt, who had better opportunities of information upon this point than any other person who has been examined, gave it as his decided opinion, that "a British subject not in the situation of ambassador, could not have been able to obtain from the Turkish government a fermaun of such extensive powers."

It may not be unworthy of remark, that the only other piece of sculpture which was ever removed from its place for the purpose of export was taken by M. Choiseul Gouffier, when he was ambassador from France to the Porte; but whether he did it by express permission, or in some less ostensible way, no means of ascertaining are within the reach of your committee. It was undoubtedly at various times an object with the French government to obtain possession of some of these valuable remains, and it is probable, according to the testimony of Lord Aberdeen, and others, that at no great distance of time they might have been removed by that government from their original site, if they had not been taken away and secured for this country by Lord Elgin.

III. The third part is involved in much less intricacy: and although in all matters of taste there is room for great variety and latitude of opinion, there will be found upon this branch of the subject much more uniformity and agreement than could have been expected. The testimony of several of the most eminent artists in this kingdom, who have been examined, rates these marbles in the very first class of ancient art, some placing them a little above, and others but very little below the Apollo Belvidere, the Laocoon, and the Torso of the Belvidere. They speak of them with admiration and enthusiasm; and notwithstanding the manifold injuries of time and weather, and those mutilations which they have sustained from the fortuitous or designed injuries of neglect, or mischief, they consider them as among the finest models, and the most exquisite monuments of antiquity. The general current of this portion of the evidence makes no doubt of referring the date of these works to the original building of the Parthenon, and to the designs of Phidias, the dawn of every thing which adorned and ennobled Greece. With this estimation of the excellence of these works it is natural to conclude that they are recommended by the same authorities as highly fit, and admirably adapted to form a school for study, to improve our national taste for the fine arts, and to diffuse a more perfect knowledge of them throughout this kingdom.

Much indeed may be reasonably hoped and expected, from the general observation, and admiration of such distinguished examples. The end of the fifteenth and beginning of the sixteenth centuries, enlightened by the discovery of several of the noblest remains of antiquity, produced in Italy an abundant harvest of the most eminent men, who made gigantic advances in the path of art, as painters, sculptors, and architects. Caught by the novelty, attracted by the beauty, and enamoured of the perfection of those newly disclosed treasures, they imbibed the genuine spirit of ancient excellence, and transfused it into their own compositions.

It is surprising to observe in the best of these marbles in how great a degree the close imitation of nature is combined with grandeur of style, while the exact details of the former in no degree detract from the effect and predominance of the latter.

The two finest single figures of this collection differ materially in this respect from the Apollo Belvidere, which may be selected as the highest and most sublime representation of ideal form and beauty, which sculpture has ever embodied, and turned into shape.

The evidence upon this part of the inquiry will be read with satisfaction and interest, both where it is immediately connected with these marbles, and where it branches out into extraneous observations, but all of them relating to the study of the antique. A reference is made by one of the witnesses to a sculptor[*], eminent throughout Europe for his works, who lately left this metropolis highly gratified by the view of these treasures of that branch of art which he has cultivated with so much success. His own letter to the Earl of Elgin upon this subject is inserted in the Appendix.

In the judgement of Mr. Payne Knight, whose valuation will be referred to in a subsequent page, the first class is not assigned to the two principal statues of this collection; but he rates the metopes in the first class of works in high relief, and knows of nothing so fine in that kind. He places also the frize in the first class of low relief; and considering a general museum of art to be very desirable, he looks upon such an addition to our national collection as likely to contribute to the improvement of the arts, and to become a very valuable acquisition; for the importation of which Lord Elgin is entitled to the gratitude of his country.

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[*] Canov

IV. The directions of the house in the order of reference impose upon your committee the task of forming and submitting an opinion upon the fourth head, which otherwise the scantiness of materials for fixing a pecuniary value, and the unwillingness, or inability, in those who are practically most conversant in statuary to afford any lights upon this part of the subject, would have rather induced them to decline.

The produce of this collection, if it should be brought to sale in separate lots, in the present depreciated state of almost every article, and more particularly of such as are of precarious and fanciful value, would probably be much inferior to what may be denominated its intrinsic value.

The mutilated state of all the larger figures, the want either of heads or features, of limbs or surface, in most of the metopes, and in a great proportion of the compartments even of the larger frize, render this collection, if divided, but little adapted to serve for the decoration of private houses. It should therefore be considered as forming a whole, and should unquestionably be kept entire as a school of art, and a study for the formation of artists. The competitors in the market, if it should be offered for sale without separation, could not be numerous. Some of the sovereigns of Europe, added to such of the great galleries or national institutions in various parts of the continent, as may possess funds at the disposal of their directors sufficient for such a purpose, would in all probability be the only purchasers.

It is not however reasonable nor becoming the liberality of parliament to withhold upon this account, whatever, under all the circumstances, may be deemed a just and adequate price; and more particularly in a case where parliament is left to fix its own valuation, and no specific sum is demanded, or even suggested, by the party who offers the collection to the public.

It is obvious that the money expended in the acquisition of any commodity is not necessarily the measure of its real value. The sum laid out in gaining possession of two articles of the same intrinsic worth, may, and often does, vary considerably. In making two excavations, for instance, of equal magnitude and labour, a broken bust or some few fragments may be discovered in the one, and a perfect statue in the other. The first cost of the broken bust and of the entire statue would in that case be the same; but it cannot be said that the value is therefore equal. In the same manner, by the loss or detention of a ship, a great charge may have been incurred, and the original outgoing excessively enhanced; but the value to the buyer will in no degree be affected by these extraneous accidents. Supposing again, artists to have been engaged at considerable salaries during a large period in which they could do little or nothing, the first cost would be burdensome in this case also to the employer; but those who bought would look only at the value of the article in the market where it might be exposed to sale, without caring, or inquiring how, or at what expense it was brought thither.

Supposing, on the other hand, that the thirteen other metopes had been bought at the Custom-house sale at the same price which that of M. Choiseul Gouffier* fetched, it could never be said that the value of them was no more than twenty-four or twenty-five pounds a-piece.

It is perfectly just and reasonable that the seller should endeavour fully to reimburse himself for all expenses, and to acquire a profit also; but it will be impossible for him to do so, whenever the disbursements have exceeded the fair money price of that which he has to dispose of.

Your committee refer to Lord Elgin's evidence for the large and heavy charges which have attended the formation of this collection, and the placing of it in its present situation; which amount, from 1799 to January 1803, to 62,440*l.* including 23,240*l.* for the interest of money; and according to a supplemental account, continued from 1803 to 1816, to no less a sum than 74,000*l.* including the same sum for interest.

All the papers which are in his possession upon this subject, including a journal of above 90 pages, of the daily expenses of his principal artist Lusieri (from 1803 to the close of 1814), who still remains in his employment at Athens, together with the account current of Messrs. Hayes of Malta (from April 1807 to May 1811), have been freely submitted to your committee; and there can be no doubt, from the inspection of those accounts, confirmed also by other testimony, that the disbursements were very considerable; but supposing them to reach the full sum at which they are calculated, your committee do not hesitate to express their opinion, that they afford no just criterion of the value of the collection, and therefore must not be taken as a just basis for estimating it.

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* M. Choiseul, the French minister at the Porte, had obtained permission to remove sculptures from Athens. The frigate which was conveying them to France was captured by Lord Nelson. The marbles were afterwards sold at a rummage sale at the Custom-house, and bought by Lord Elgin, who then thought them a part of his own collection, the packages having no directions.—Edit.

Two valuations, and only two in detail, have been laid before your committee, which are printed; differing most widely in the particulars, and in the total; that of Mr. Payne Knight amounting to 25,000*l.* and that of Mr. Hamilton to 60,800*l.*

The only other sum mentioned as a money price, is in the evidence of the Earl of Aberdeen, who named 35,000*l.* as a sort of conjectural estimate of the whole, without entering into particulars.

In addition to the instances of prices quoted in Mr. Payne Knight's evidence, the sums paid for other celebrated marbles deserve to be brought under the notice of the house.

The Townley collection, which was purchased for the British Museum in June 1805 for 20,000*l.*, is frequently referred to in the examinations of the witnesses, with some variety of opinion as to its intrinsic value; but it is to be observed of all the principal sculptures in that collection, that they were in excellent condition with the surface perfect; and where injured, they were generally well restored, and perfectly adapted for the decoration and almost for the ornamental furniture of a private house, as they were indeed disposed by Mr. Townley in his lifetime.

In what proportion the state of mutilation in which the Elgin marbles are left, and above all the corrosion of much of the surface by weather reduce their, value, it is difficult precisely to ascertain; but it may unquestionably be affirmed in the words of one of the sculptors* examined (who rates these works in the highest class of art) that "the Townleyan marbles being entire, are, in a commercial point of view, the more valuable of the two: but that the Elgin marbles, as possessing that matter which artists most require, claim a higher consideration."

The Ægina marbles, which are also referred to, and were well known to one of the members of your committee†, who was in treaty to purchase them for the British Museum, sold for 6,000*l.* to the Prince Royal of Bavaria, which was less than the British government had directed to be offered, after a prior negotiation for obtaining them had failed; their real value however was supposed not to exceed 4000*l.* at which Lusieri estimated them. They are described as valuable in point of remote antiquity, and curious in that respect, but of no distinguished merit as specimens of sculpture, their style being what is usually called Etruscan, and older than the age of Phidias.

The marbles at Phigalia, in Arcadia, have lately been purchased for the Museum at the expense of 15,000*l.* increased by a very unfavourable exchange to 19,000*l.* a sum which your committee, after inspecting them, venture to consider as more than equal to their value.

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* R. Westmacott, esq. R.A.

† J. N. Fazakerley, esq.

It is true that an English gentleman*, concerned in discovering them, was ready to give the same sum; and therefore no sort of censure can attach on those who purchased them abroad, for our national gallery, without any possible opportunity of viewing and examining the sculpture, but knowing them only from the sketches which were sent over, and the place where they were dug up, to be undoubted and authentic remains of Greek artists of the best time.

When the first offer was made by the Earl of Elgin to Mr. Perceval, of putting the public in possession of this collection, Mr. Long, a member of your committee, was authorized by Mr. Perceval to acquaint Lord Elgin, that he was willing to propose to parliament to purchase it for 30,000*l.* provided Lord Elgin should make out, to the satisfaction of a committee of the house of commons, that he had expended so much in acquiring and transporting it.

Lord Elgin declined this proposal, for the reasons stated by him in his evidence: and until the month of June 1815 no further step was taken on either side; but at that time a petition was presented, on the part of Lord Elgin, to the house, which, owing to the late period of the session, was not proceeded upon. Eighty additional cases have been received since 1811, the contents of which, enumerated in Mr. Hamilton's evidence, now form a part of the collection. The medals also, of which the value is more easily defined, were not included in the proposal made to Mr. Perceval.

Against these augmentations must be set the rise in the value of money, which is unquestionably not inconsiderable, between the present time and the year 1811; a cause or consequence of which is the depreciation of every commodity, either of necessity or fancy, which is brought to sale.

Your committee, therefore, do not think that they should be justified, in behalf of the public, if they were to recommend to the house any extension of Mr. Perceval's offer to a greater amount than 5000*l.*: and, under all the circumstances that they have endeavoured to bring under the view of the house, they judge thirty-five thousand pounds to be a reasonable and sufficient price for this collection.

Your committee observing, that by the act 45 Geo. III. c. 127, for vesting the Townleyan collection in the trustees of the British Museum, § 4, the proprietor of that collection, Mr. Townley Standish, was added to the trustees of the British Museum, consider the Earl of Elgin (and his heirs being Earls of Elgin) as equally entitled to the same distinction, and recommend that a clause should be inserted to that effect, if it should be necessary that an act should pass for transferring his collection to the public.

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* Mr. Lee, of Warwickshire.

It may not be deemed foreign to this subject, if your committee venture to extend their observations somewhat beyond the strict limit of their immediate inquiry, and lay before the house what occurs to them as not unimportant with regard to the age and authenticity of these sculptures. The great works with which Pericles adorned and strengthened Athens, were all carried on under the direction and superintendance of Phidias: for this, there is the authority of various ancient writers, and particularly of Plutarch; but he distinctly asserts in the same passage, that Callicrates and Ictinus executed the work of the Parthenon; which is confirmed also by Pausanias, so far as relates to Ictinus, who likewise ornamented or constructed the temple of Apollo at Phigalīa*; from whence, by a singular coincidence, the sculptu[illegible] in high relief lately purchased for the British Museum, and frequently referred to in the evidence, were transported.

The style of this work, in the opinion of the artists, indicates that it belongs to the same period, though the execution is rated as inferior to that of the Elgin marbles. In the fabulous stories which are represented upon both, there is a very striking similarity; and it may be remarked in passing, that the subjects of the metopes, and of the smaller frize, which is sculptured with the battle of the Amazons, correspond with two out of the four subjects mentioned by Pliny, as adorning the shield and dress of the Minerva; so that there was a general uniformity of design in the stories which were selected for the internal and external decoration of the Parthenon. The taste of the same artist, Ictinus, probably led him to repeat the same ideas, which abound in graceful forms, and variety of composition, when he was employed upon the temple of another divinity at a distance from Athens.

The statue of Minerva, within the temple, was the work of Phidias himself, and, with the exception of the Jupiter which he made at Elis, the most celebrated of his productions. It was composed of ivory and gold; with regard to which, some very curious anecdotes relating to the political history of that time are to be found in the same writers: the earliest of which, from a passage in a cotemporary poet, Aristophanes, proves that the value of these materials involved both Pericles and the director of his works in great trouble and jeopardy; upon which account the latter is said to have withdrawn to Elis, and to have ended his days there, leaving it doubtful whether his death was natural, or in consequence of a judicial sentence: but Plutarch places his death at Athens, and in prison, either by disease or by poison.

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* The penultimate syllable should be pronounced long; Phigaīla closes two hexameter verses, one of whi[illegible] quoted by Pausanias, and the other by Stephanus Byzantinus, from Rhianus a poet of Crete.

It has been doubted whether Phidias himself ever wrought in marble; but although, when he did not use ivory, his chief material was unquestionably bronze; there are authorities sufficient to establish, beyond all controversy, that he sometimes applied his hand to marble. Pliny, for instance, asserts that he did so, and mentions a Venus ascribed to him, existing in his own time in the collection (or in the portico) of Octavia. Phidias is called by Aristotle a skilful worker in stone; and Pausanias enumerates a celestial Venus of Parian marble, undoubtedly of his hand; and the Rhamnusian Nemesis, also of the same material. Some of his statues in bronze were brought to Rome by Paulus Æmilius, and by Catulus.

His great reputation, however, was founded upon his representations of the gods, in which he was supposed more excellent than in human forms, and especially upon his works in ivory, in which he stood unrivalled[*].

Elidas the Argive is mentioned as the master of Phidias; which honour is also shared by Hippias. His two most celebrated scholars were Alcamenes an Athenian of noble birth, and Agoracritus of Paros; the latter of whom was his favourite; and it was reported, that out of affection to him, Phidias put his scholar's name upon several of his own works: among which the statue called Rhamnusian Nemesis is particularized by Pliny and Suidas.

In another passage of Pliny, Alcamenes is classed with Critias, Nestocles, and Hegias, who are called the rivals of Phidias. The name of Colotes is preserved as another of his scholars.

The other great sculptors who were living at the same time with Phidias, and flourished very soon after him, were Agelades, Callon, Polycletus, Phragmon, Gorgias, Lacon, Myron, Pythagoras, Scopas, and Perclius.

The passage in which Pausanias mentions the sculptures on the pediments is extremely short, and to this effect: "As you enter the temple which they call Parthenon, all that is contained in what is termed the (*eagles*) pediments, relates in every particular to the birth of Minerva; but on the opposite or back front is the contest of Minerva and Neptune for the land;—but the statue itself is formed of ivory and gold." The state of dilapidation into which this temple was fallen when Stuart visited it in 1751, and made most correct drawings for his valuable work, left little opportunity of examining and comparing what remained upon that part of the temple with the passage referred to: but an account is preserved by travellers, who about eighty years earlier found one of these pediments in tolerable preservation, before the war between the Turks and Venetians, in 1687, had done so much damage to this admirable structure. The observations of one of these (Dr. Spon a French physician) may be literally translated thus:

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[*] Quintilian xii. c. 10

"The highest part of the front which the Greeks called 'the Eagle,' and our architects 'the Fronton,' is enriched with a group of beautiful figures in marble, which appear from below as large as life. They are of entire relief, and wonderfully well worked. Pausanias says nothing more, than that this sculpture related to the birth of Minerva. The general design is this:

"Jupiter, who is under the highest angle of the pediment (fronton) has the right arm broken, in which, probably, he held his thunderbolt; his legs are thrown wide from each other, without doubt to make room for his eagle. Although these two characteristics are wanting, one cannot avoid recognising him by his beard, and by the majesty with which the sculptor has invested him. He is naked, as they usually represented him, and particularly the Greeks, who for the most part made their figures naked; on his right is a statue which has its head and arms mutilated, draped to about half the leg, which one may judge to be a Victory, which precedes the car of Minerva, whose horses she leads. They are the work of some hand as bold as it was delicate, which would not perhaps have yielded to Phidias, or Praxiteles, so renowned for (representing) horses. Minerva is sitting upon the car, rather in the habit of goddess of the sciences, than of war; for she is not dressed as a warrior, having neither helmet, nor shield, nor head of Medusa upon her breast: she has the air of youth, and her head-dress is not different from that of Venus. Another female figure without a head is sitting behind her with a child, which she holds upon her knees. I cannot say who she is; but I had no trouble in making out or recognising the two next, which are the last on that side; it is the emperor Hadrian sitting, and half naked, and, next to him, his wife Sabina. It seems that they are both looking on with pleasure at the triumph of the goddess. I do not believe that before me any person observed this particularity, which deserves to be remarked. On the left of Jupiter are five or six figures, of which some have lost the heads; it is probably the circle of the gods, where Jupiter is about to introduce Minerva, and to make her be acknowledged for his daughter.' The pediment behind represented, according to the same author, the dispute which Minerva and Neptune had for naming the city; but all the figures are fallen from them, except one head of a sea-horse, which was the usual accompaniment of this god: these figures of the two pediments were not so ancient as the body of the temple built by Pericles, for which there wants no other argument than that of the statue of Hadrian, which is to be seen there, and the marble which is whiter than the rest. All the rest has not been touched. The Marquis de Nointel had designs made of the whole, when he went to Athens; his painter worked there for two months, and almost lost his eyes, because he was obliged to draw every thing from below, without a scaffold."—(*Voyage par Jacob Spon:* Lyons, 1678; 2 tom. p. 144.)

Wheler, who travelled with Spon, and published his Work at London (four years later) in 1682, says, "But my companion made me observe the next two figures sitting in the corner to be of the emperor Hadrian and his empress Sabina, whom I easily knew to be so, by the many medals and statues I have seen of them." And again, "But the emperor Hadrian most probably repaired it, and adorned it with those figures at each front. For the whiteness of the marble, and his own statue joined with them, apparently show them to be of a later age than the first, and done by that emperor's command. Within the portico on high, and on the outside of the cella of the temple itself, is another border of basso relievo round about it, or at least on the north and south sides, which, without doubt, is as ancient as the temple, and of admirable work, but not so high a relievo as the other. Thereon are represented sacrifices, processions, and other ceremonies of the heathens' worship; most of them were designed by the Marquis de Nointel, who employed a painter to do it two months together, and showed them to us when we waited on him at Constantinople."

Another French author, who published, three years earlier than Spon, a work called "Athènes Ancienne et Nouvelle, par le S^r de la Guilletiere à Paris," 1675,—says, "Pericles employed upon the Parthenon the celebrated architects Callicrates and Ictinus. The last, who had more reputation than the former, wrote a description of it in a book*, which he composed on purpose, and which has been lost; and we should probably not now have the opportunity of admiring the building itself, if the emperor Hadrian had not preserved it to us, by the repairs which he caused to be done. It is to his care that we owe the few remains of antiquity which are still entire at Athens."

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* Ictinus and Carpion were [illegible] concerned in this work, for which we have the authority of Vitruvius, lib. 7. præfat.

In the Antiquities of Athens, by Stuart, vol. ii. p. 4, it is said, "Pausanias gives but a transient account of this temple, nor does he say whether Hadrian repaired it, though his statue and that of his empress Sabina in the western pediment have occasioned a doubt whether the sculptures, in both, were not put up by him. Wheler and Spon were of this opinion, and say they were whiter than the rest of the building. The statue of Antinous, now remaining at Rome, may be thought a proof that there were artists in his time capable of executing them; but this whiteness is no proof that they were more modern than the temple, for they might be made of a whiter marble; and the heads of Hadrian and Sabina might be put on two of the ancient figures, which was no uncommon practice among the Romans; and if we may give credit to Plutarch, the buildings of Pericles were not in the least impaired by age in his time; therefore, this temple could not want any material repairs in the reign of Hadrian."

With regard to the works of Hadrian at Athens, Spartian says "that he did much for the Athenians*;" and a little after, on his second visit to Athens, "going to the east he made his journey through Athens, and dedicated the works which he had begun there; and particularly a temple to Olympian Jupiter, and an altar to himself."

The account given by Dion Cassius is nearly to the same effect, adding, that he placed his own statue within the temple of Olympian Jupiter, which he erected†.

He called some other cities after his own name, and directed a part of Athens to be styled Hadrianopolis‡: but no mention is made by any ancient author, of his touching or repairing the Parthenon. Pausanias, who wrote in his reign, says that "the temples which Hadrian either erected from the foundation, or adorned with dedicated gifts and decorations, or whatever donations he made to the cities of the Greeks, and of the Barbarians also, who made application to him, were all recorded at Athens in the temple common to all the gods§."

It is not unlikely, that a confused recollection of the statue which Hadrian actually placed at Athens, may have led one of the earliest travellers into a mistake, which has been repeated and countenanced by subsequent writers: but Mr. Fauvel, who will be quoted presently, speaks as from his own examination

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[*] Folio edit Paris 1620, p. 6. † b lxix. c. 16.

[‡] Spartian, p. 10. § Paus. Att. p. 5. Ed. Xyl.

and observation, when he mentions the two statues in question; which, it is to be observed, still remain (without their heads) upon the pediment of the entrance, and have not been removed by Lord Elgin.

An exact copy of these drawings, by the Marquis de Nointel's painter, is given in M. Barry's works; which are rendered more valuable on account of the destruction of a considerable part of the temple in the Turkish war by the falling of a Venetian bomb, within a short time after the year in which they were made; which, however, must have been prior to the date of 1683, affixed to the plate in Barry's Works (2d vol. p. 163. London, 1809.)

Some notes of Mr. Fauvel, a painter and antiquarian, who moulded and took casts from the greatest part of the sculptures, and remained fifteen years at Athens, are given with the tracings of these drawings; in which it is said, with regard to these pediments, "these figures were adorned with bronze, at least if we may judge by the head of Sabina, which is one of the two that remain; and which, having fallen, and being much mutilated, was brought to Mr. Fauvel. The traces are visible of the little cramps which probably fixed the crown to the head. The head of the emperior Hadrian still exists. Probably this group has been inserted to do honour to that emperor, for it is of a workmanship different from the rest of this sculpture."

Your committee cannot dismiss this interesting subject, without submitting to the attentive reflection of the house, how highly the cultivation of the fine arts has contributed to the reputation, character, and dignity of every government by which they have been encouraged, and how intimately they are connected with the advancement of every thing valuable in science, literature, and philosophy. In contemplating the importance and splendor to which so small a republic as Athens rose, by the genius and energy of her citizens, exerted in the path of such studies, it is impossible to overlook how transient the memory and fame of extended empires and of mighty conquerors are, in comparison of those who have rendered inconsiderable states eminent, and immortalized their own names by these pursuits. But if it be true, as we learn from history and experience, that free governments afford a soil most suitable to the production of native talent, to the maturing of the powers of the human mind, and to the growth of every species of excellence, by opening to merit the prospect of reward and distinction, no country can be better adapted than our own to afford an honourable asylum to these monuments of the school of Phidias, and of the administration of Pericles; where, secure from further injury and degradation,

gradation, they may receive that admiration and homage to which they are entitled, and serve in return as models and examples to those, who by knowing how to revere and appreciate them, may learn first to imitate, and ultimately to rival them.

March 25. 1816.

⁂ The witnesses examined were: the Earl of Elgin; the Right Hon. Charles Long; William Hamilton, Esq.; Joseph Nollekens, Esq. R.A.; John Flaxman, Esq. R.A.; Richard Westmacott, Esq.; Francis Chauntry, Esq.; Charles Rossi, Esq. R.A.; Sir Thomas Lawrence, Knt. R.A.; Richard Payne Knight, Esq.; William Wilkins, Esq.; Taylor Combe, Esq.; the Earl of Aberdeen; John Bacon, Esq.; J. B. Sawrey Morritt, Esq.; John Nicholas Fazakerley, Esq.; Alex. Day, Esq.; Rev. Dr. Philip Hunt, LL.D. Questions in writing were also sent to the President of the Royal Academy, who, owing to indisposition, could not wait upon the committee; and these with his answers thereto are also inserted in the Appendix.

All the artists examined spoke in the most enthusiastic terms of these noble specimens of art: most of them preferred the Theseus and the Neptune even to the Belvidere Apollo, and the Laocoon; and all of them agreed that the collection was finer than any thing they had ever seen.

The Phygalian Marbles.

The following particulars relative to the Phygalian and Ægina marbles are extracted from Mr. Hamilton's evidence before the committee on the Elgin marbles.

"Are you acquainted with the transaction relating to the purchase of the Phygalian marbles?—Yes, I am; the best information I can give to the committee, on the subject of the purchase of the Phygalian marbles, is contained in a memorandum, the copy of which I put into Mr. Long's hands about ten days ago: This is the paper. [It was read as follows.]

"Memorandum on the purchase of the Phygalian Marbles on account of the British Government.

"When the first intelligence of the discovery of the Phygalian marbles, by a party of English and German travellers, in the month of * 1812, was received in England, I heard, owing to my intimacy with the family of Mr. Cockerell, father of one of the fortunate discoverers, frequent and detailed accounts of the beauty of these remains of antiquity, and the extraordinary state of preservation in which they had been found, notwithstanding the lapse of more than twenty centuries since they had been sculptured. In that and the subsequent year, drawings of the bas-reliefs were received in England by various hands, particularly some very correct ones by Mr. C. R. Cockerell, brought by Mr. Frederick North, all attesting the beauty of the composition, and eminently satisfactory with regard to the age in which they had been made. These drawings I saw frequently exhibited to persons the most competent to form a judgement of the merit of the originals; and they met with universal admiration, both in general society, and particularly at the meetings of the Dilettanti Society. It was on all hands hoped that they might be purchased by the British government, and that they would not be deterred by the bad success of the negotiation for the Ægina marbles, from becoming competitors also for these: these feelings were also expressed by several of the trustees of the British Museum, but in such general terms, that I was not very sanguine of what seemed to be the wish of all being brought about by the efficient co-operation of a few; though I was aware that this offered the only chance of success. Perhaps the failure of the two successive attempts, which had been made for the purchase of the Ægina marbles,* damped, in some measure, the disposition of those who, from their public situation, and correct judgement in all matters of taste, were qualified and entitled to interfere. However it was, the time for the public sale announced for the 1st May 1814 was fast approaching, and no steps were taken for the attainment of the object, of which I was aware, beyond a few visits, which I received about that time from General Turner, to express the hopes of the Prince Regent, to whom the drawings, brought home by Mr. North, had been submitted by Mr. Cockerell, the father, that the marbles in question would be purchased; and from Mr. Planta, to express the same hopes on the part of the British Museum, though unauthorized officially by the trustees.

"With regard to the supposed value of these marbles, as none had been seen in England, and scarcely any traveller of taste or judgement who had seen them at Corfu, except Mr. North, had given his opinion in this country as to their relative or comparative merit; the only criterions that any one could go by were, first, a comparison between the drawings of them and the original works of Phidias in the Elgin collection; and secondly, the price put upon them by the proprietors, below which it was formally declared that they would not be parted with; and a sum equal to which I was assured that one of the proprietors had offered to give, if the public sale could be dispensed with, or if no larger sum were offered. His price was 15,000*l.* or 60,000 Spanish dollars; the collection might in fact be worth that sum, or more or less; it was not possible to anticipate. However, I felt confident, from the degree of merit which it was evident they must possess, at the sight of drawings sent home by Mr. R. Cockerell, a gentleman incapable of disguise, as well as from the interest which must necessarily be felt in every work of Grecian art executed in the age of Pericles, or at least in that immediately subsequent; considering likewise the general disappointment and regret which would be felt if the moment were lost, and they should irrecoverably get into the hands of one of the continental sovereigns; I was convinced that it would be desirable for the cause of the arts in England, that the purchase should, if possible, be effected.

"Lord Castlereagh being at this time absent on the continent, I applied forthwith to the first Lord of the Treasury, the Chancellor of the Exchequer, and the Colonial Secretary of State; and on laying before them the above considerations, I received from them severally their consent, that the governor of Zante should be authorised to effect the purchase at a public sale to the amount mentioned. A messenger was immediately sent off, who arrived a few days previous to the sale, and the bargain was concluded for 60,000 dollars.

"Was the purchase effected at 15,000*l.*?—The price was 60,000 dollars; by the course of exchange it came to 19,000*l.*

"To what circumstance was it owing, a public sale could not be dispensed with?—Because the property belonged half to Germans and half to Englishmen, and they would not allow any one, even of the discoverers, to make the purchase without a public sale. Mr. Lee, one of the Englishmen, a gentleman of large fortune in Warwickshire, I was assured, offered the money if he was allowed to take them without a public sale, and I have that in Mr. Cockerell's hand-writing.

"Do you know what the expense of bringing them to England was?—No, I do not; they came over in a ship of war or a transport, therefore I should think the expense would be very little.

"You mentioned that the public were disappointed respecting the Ægina marbles; in what way was that?—They were discovered about two years before, by two English travellers and two German travellers. Mr. Cockerell was one of the English discoverers, and he wrote a detailed account of it home to his father, and mentioned, that the value they set upon them at Athens at that time was 6000*l.* This being communicated, and being the subject of conversation at the Dilettanti Society, Lord Hardwicke, who is a member of that Society and a trustee of the British Museum, undertook to recommend to the trustees of the British Mueseum, to request the authority of Government to make an offer of 6000*l.* The offer was made in the first instance through Mr. Cockerell, but on these conditions, that we should be allowed to bring home the marbles to England, and if they were found worth 6,000*l.* that we should have the refusal of them; if not, they should be allowed to be exported, free of duty, for any other purchaser. This offer having arrived at Athens, was not accepted; for they said it was a kind of blind bargain; that they did not know what might become of them. Afterwards the British Museum sent out Mr. Combe, the superintendant of antiquities, to Malta, to bid 8,000*l.* at a sale of them expected to take place on the first of November. He arrived a few days before that date; he waited the month of November, but no sale took place, and he left his commission with the governor of the island; but in the mean time a private sale had taken place at Zante to the Prince Royal of Bavaria; but notwithstanding they were sold to the Prince Royal of Bavaria, they were conveyed for a few months to Malta, for greater security: and there was a considerable difference of opinion whether we ought not to have insisted upon a second sale, having been disappointed in the first sale not having taken place at Malta as it was publicly announced; but it was ultimately determined to give up the matter.

"Can you state what sum the Prince Royal of Bavaria gave for those marbles?—I understood 6,000*l.*

"Do you know of what those Ægina marbles consisted?—I think there were seventeen figures with sixteen heads, which were found under the two pediments of the temple of Jupiter at Ægina.

"Of what proportions were the figures?—I should say between three and four feet."

LIX. *On certain Electrical Phænomena. By* Thos. Howldy, *Esq. in Reply to Mr.* Donovan.

Sir,—Mr. Donovan has pointed out in the number of your Magazine for March, an inaccuracy of mine concerning his "Reflections on the Inadequacy of Electrical Hypotheses." In the conclusion of my examination of some experiments of that gentleman, inserted in your number for December, I inadvertently stated that the "Reflections," had obtained a prize from the Royal Irish Academy. I find on consulting the page and volume of your Magazine to which I referred in *a note accompanying my statement*, that it was his "Essay on the Origin, &c. of Galvanism," and not the "Reflections," that was honoured with the prize. Should this unintentional mis-statement have been in the smallest degree offensive to the Royal Irish Academy or to Mr. Donovan, I beg leave, sir, to express my regret at its occurrence. I must at the same time observe, that Mr. Donovan has also been guilty of an inadvertency, having most strangely, but I cannot believe wilfully, misrepresented the meaning conveyed in the first part of the sentence with which my paper concludes, as will evidently appear by comparing it with the first two sentences in the second paragraph of his answer.

I now proceed to reply to the observations by which he attempts to support the correctness of his experiments, and to invalidate the results of those which I have opposed to them. "Extensive reading would have shown that the electrical states attributed in my 'Reflections' to the Leyden phial had not been noticed by me alone.

"Experimenters of great reputation had observed analogous facts, and of this a perusal of the works of Wilson, Eeles, of the Encyclopædia Britannica (art. Electricity), and of various other authorities that I now forget, will afford ample testimony. &c." Here it would have been well, if, instead of making this vague reference to authority, Mr. Donovan had specified *some* of the facts which "experimenters of great reputation had observed," and had shown in what manner and to what extent they support *his* experiments. That he should have referred to Eeles is singular; for, in the first place, he has in his "Reflections" combated that writer's hypothesis; and secondly, as he must be aware, his own experiments (supposing them correct) are as irreconcilable to that hypothesis, as they are to Franklin's theory. It does not appear therefore that Eeles can render any assistance to Mr. Donovan on this occasion; and if he could have derived any from the other sources he has mentioned, it is pity he has not availed himself of it. "The question does not relate merely to the states of the phial, but comprises the whole doctrine of *plus* and *minus*." Mr. Donovan's memory appears here to have failed him; for the objections he has urged in his "Reflections" against the doctrine of accumulation and deficiency, are derived principally from his examination of the electrical states of the phial; and accordingly it was those objections alone that I undertook to refute. Hence there is little propriety in the following questions which he puts in his answer. "Why should so limited a survey of those objections be taken, which are all in harmony; and why need one part of the hypothesis be sustained when it is opposed by the other?" He includes here in the term "objections" not only those he had made in his "Reflections," but also those "analogous facts" which, he says, "experimenters of great reputation had observed;" but which he *did not there specify and propose as such*; although in his answer he lumps theirs and his own together,

gether, and then complains that in my "limited survey" I did not notice the objections of both; when he knows that in his "Reflections" he stated only his own objections but none of theirs, and consequently I had only the former to notice. "The experiments are of a delicate kind, and I stated that they only succeed in certain states of the weather. The period when I made them was dry sunny weather, and I have never found them to succeed easily but in the middle of summer." Mr. Donovan in the above sentences has endeavoured to define the *state of the weather* in which his *delicate* experiments will succeed; but unfortunately he has not accomplished his intention. "The period when he made them, he says, was dry and sunny." Now we may have dry and sunny weather in spring, summer, autumn, and winter; and consequently it might have been inferred that in such a case the experiments might be made in either of those seasons: but this inference seems to be doubtful; for he immediately adds, "I have never found them to succeed *easily* but in the middle of summer." Here, though the time or season of the year is determined, the state of the weather is left undetermined; for the middle of summer is not always dry and sunny, but is sometimes dry and cloudy, and sometimes wet and cloudy. This uncertainty with respect to the weather seems to have prevented Mr. Donovan from repeating his experiments; but when he receives the intelligence that they may *with a little care* be successfully performed in *every state of the weather*, on *any day in the year*, and at *any hour of the day*, he will doubtless be much gratified to find that he needs not wait till midsummer to ascertain the fallacy of his own, or the correctness of mine. "It is very difficult to determine what body is positive or what negative; for positive bodies will, under certain circumstances, attract positive bodies, and negative bodies will attract negative." I deny that it is very difficult to determine what body is positive or what negative, for in most cases it is extremely easy to determine it; and if Mr. Donovan has really experienced any difficulty in such cases, he has experienced it where no other electrician has experienced any." If he alludes particularly to the experiments in dispute, I still deny the fact; for there are several methods by which the electrical states of the phial may be ascertained with ease and certainty. The reason that he assigns for the above difficulty is, that "positive bodies will, under certain circumstances, attract positive bodies; and negative bodies will attract negative." Let this be granted, and I ask, will positive bodies *repel negative bodies*, or negative bodies *repel positive bodies*, under any circumstances whatever [illegible] if they will not, this difficulty of Mr. Donovan's must vanish in [illegible]ment. "The electrometer of Bennet is an instrument not to be depended on without acquaintance with a principle of electricity which I have developed, and which in a work soon to appear will be shown to have misled many able investigators. Any electrometer on the same principle, whether pith-balls or gold-leaf, is liable to the same objection. In the counter-experiments this error has not been guarded against." Mr. Donovan, not satisfied with the defence he has made for his experiments, here turns short round upon the innocent electrometers, and tells us they cannot be depended on; and yet, I believe that any of them may be more safely depended on than the sulphur on which he has been pleased to place his whole reliance. However, if he continues obstinate, and is determined to reject their testimony, I have no objection to his so doing; for, on re-perusing my paper, he will find that the excited glass tube by always steadily repelling the balls connected with the overcharged surface of the jar, and that the excited wax by always steadily repelling those connected with the undercharged surface, afford ample evidence in my favour independently of the testimony of the reprobated electrometers. Equally steady in its testimony was the small Leyden phial I employed, and the electrometers were only used to ascertain the electric changes which took place in the excited electrics: hence Mr. Donovan's attack upon the electrometers avails him nothing, and there is little truth in the remark that "in the counter-experiments this error has not been guarded against;" for, had the electrometers been as treacherous as he would wish us to believe, and had I been as much deceived by employing them in my experiments as he was by *not* employing them in his, my errors would still have had no relation to the electrical states of the phial; because they were ascertained by the faithful and unimpeached testimony of the other instruments.

I am, sir,
Your obliged servant,
Hereford, April 15, 1816. **Thomas Howldy.**

LX. *On preserving Potatoes for Sea Store or Exportation, by Mr.* Charles Whitlow, *of Canada; and on preserving Carrots, by* H. B. Way, *Esq. of Bridport Harbour*[*].

New York Coffee-House, Feb. 12, 1815.

Sir,—The usual mode at present practised for endeavouring to preserve potatoes, is to leave them after digging exposed to the sun and air until they are dry: this exposure generally causes them to have a bitter taste, and it may be remarked, that potatoes are never so sweet to the palate as when cooked immediately after digging. I find that when potatoes are left in large heaps or pits in the ground, that a fermentation takes place which destroys the sweet flavour of the potatoes. In order to prevent that fermentation, and to preserve them from losing the original fine and pleasant flavour, my plan is (and which experience proves to me to have the desired effect) to have them packed in casks as they are digging from the ground, and to have the casks, when the potatoes are piled in them, filled up with sand or earth, taking care that is done as speedily as possible, and that all vacant spaces in the cask are filled up by the earth or sand: the cask thus packed holds as many potatoes as it would was the earth or sand used in the packing; and as the vacant spaces of the cask of potatoes so packed are filled, the air is totally excluded, and cannot act on the potatoes, and consequently no fermentation can take place.

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[*] From *Transactions of the Society for the Encouragement of Arts, Manufactures and Commerce*, vol. xxxiii. for 1815.—The silver medal of the Society was voted for this communication.

I sailed from New York to St. Bartholomew's, and brought with me two hundred barrels of potatoes packed in the above manner: on my arrival at the island I found, as I expected, that the potatoes had preserved all their original sweetness of flavour, in fact, as good as when first dug, having undergone no fermentation, nor in the slightest degree affected by the bilge or close air of the ship. Some barrels of the potatoes I sold there, and at the neighbouring islands, for four dollars per bushel: at the same time potatoes taken out in bulk without packing, and others that were brought there packed in casks which had not been filled up with earth, sold only for one dollar per bushel, they being injured in the passage by the bilged air and fermentation, being bitter and bad, whilst mine were as perfectly sweet and dry as when dug: what remained I shipped from St. Bartholomew's to Jamaica, where they arrived in equal good condition, and sold at a higher price than they had brought at the former island: some of these casks of potatoes were put in a cool cellar by the purchaser at Jamaica; and on examining them when I was leaving the island two months after, I found that they had, in a very small degree, sprouted, but that all their original flavour was preserved. Reflecting seriously on this discovery, suggested to my mind the idea of proposing to the British nation a mode of supplying their West India colonies with a good and wholesome food for the negroes, and also for the white people, and which would find an additional market for the farmer at home, a valuable freight for the merchant, and a more extended market for the lumber of the North American colonies, viz. of Canada, Nova Scotia, &c.

It is well known that our ships in the West India trade in general go out in ballast, or not more than one-third freighted, carrying out some small quantity of European commodities; but the bulk of their freight consisting of empty casks, and materials for making casks. It is also well known how valuable a food potatoes are in the West India islands, and how much they are prized there: no one acquainted with the West Indies and its commerce, but must be aware how much labour of the unfortunate negro is at present employed in making casks, puncheons, &c. for bringing home the produce, and of what immense value casks are there. Let timber imported from our North American colonies be made into casks, hogsheads, rum puncheons, coffee barrels, &c. &c. let these be filled in my mode, as described, with potatoes, I contend, that the value of the casks which brings out potatoes will more than compensate for their freight, and the earth will keep the cask perfectly sweet, and ready, without any labour, to bring home any produce.

The potatoes must come cheap to market, the ship owner can afford cheap freight, having now none, or next to none, for his outward-bound vessels.

The farmers on the sea-coast can easily supply more than two hundred thousand tons of potatoes, and the population of the West India islands would consume more than that quantity.

Any overplus required may be readily supplied in like manner in Nova Scotia, Cape Breton, Canada, &c. The food of the negro is at present Indian corn and meal, which, with a small quantity of potatoes now used in the islands, was formerly principally supplied by the United States, who receive in return, in cash and produce, nearly ten millions of dollars. Potatoes and fish, together with the produce of the islands, will give a much more wholesome food, in a greater abundance, and at a more reasonable rate.

The policy of our legislature surely should be to encourage the parent state and the colonies, supplying each other in every possible manner, and to discourage aliens from reaping advantages from British capital, industry, and exertion, more particularly so when by judicious arrangement both the colonies and the mother country can have their wants supplied better from their superabundant productions than from foreign states. Proper encouragement for the fisheries of Newfoundland, with settlements for those employed in that part of the service on the coast of our settlements in North America, is indispensably necessary—markets there are in abundance for the employment of more ships and seamen than we have now in that trade.

By my plan of preserving potatoes, a wholesome food will be provided for the West India islands, much better and cheaper than they possess at present; a valuable freight for our outward-ward-bound shipping, which they now want; that by doing so will in some measure enable the merchant to have his return freight cheaper, and thus we do away with the idea of having our islands dependent on the American States for food. We save an immense sum of money annually given to foreigners; and to the man of humane feeling it must be a source of gratification, to see that by this mode the severe labour of the poor black is much lightened, his condition ameliorated; and by having less occasion for his labour, aiding to abolish that horrid traffic of the slave trade. We find a market never before discovered for our agricultural exertions, giving healthful and beneficial employment to many families at home and abroad, and a saving to the nation of at least five hundred thousand pounds annually.

CHARLES WHITLOW.

To C. Taylor, M.D. Sec.

A communication from H. B. WAY, Esq. (printed in the same volume, and for which the Society voted the Ceres silver medal,) on the preservation of carrots during winter, corroborates the above plan for preserving roots. The following is the substance of Mr. Way's communication:—

His carrots were sown broadcast in the usual way, in his garden, March 23d, 1814, and thinned out as wanted for family use; and on the 20th of August following they were all dug up, the greens and tops of the roots cut off and cleared from the earth that adhered to them, and were immediately put in a dry cask, first laying a layer of earth on the bottom of the cask, and then a layer of carrots and earth alternately, till the whole were put in. The cask being covered was then placed in a dry cellar, and remained there till sent to the Society in March 1815. Carrots preserved in this way are vastly superior to carrots that remain in the ground till the latter end of September or October, and then taken up and preserved; but they require nearly double the time in boiling that carrots do when immediately taken out of the ground.

It appeared to the committee, on minute examination, that the means adopted by Mr. Way for preserving carrots, is fully adequate to the purpose; and that where an opportunity offers of procuring proper casks, carrots may be preserved for many months in this mode, either for exportation or home use, in a perfectly sound state.

LXI. *Notices respecting New Books.*

Chemical Essays, principally relating to the Arts and Manufactures of the British Dominions. By SAMUEL PARKES, F.L.S., Member of the Geological Society, &c. in Five Vols., with Twenty-three Copper-plate Engravings. Price 2*l.* 2*s.*

IN our last volume, p. 48, we gave an article on the Citric Acid, extracted from this work, and in a note promised to lay before our readers some further account of this very interesting and entertaining production. We hoped to have done so before this time; but have been prevented by a pressure of temporary subjects which would have lost much of their utility by a delay; and besides, Mr. Parkes's work contains so large a mass of curious and original matter, that it required some time to make such a selection as might render justice to so useful, judicious, and laborious a writer.

The work consists of Fifteen distinct Essays on the following subjects:

I. On the Utility of Chemistry to the Arts and Manufactures of Great Britain.—II. On Temperature.—III. On the various methods of ascertaining the Specific Gravity of Bodies.—IV. On Calico Printing.—V. On Barytic Earth.—VI. On Carbon.—VII. On the Manufacture of Sulphuric Acid.—VIII. On the Preparation and Uses of Citric Acid.—IX. On the fixed Alkalies.—X. On the Manufacture of Earthen Ware and Porcelain.—XI. On the Manufacture of the different kinds of Glass.—XII. On the Art and Practice of Bleaching.—XIII. On Water, and the various Methods of its Purification.—XIV. On the Manufacture of Sal Ammoniac.—XV. On Edge Tools, and the methods of tempering them.

The last volume of this interesting publication contains 250 additional notes, which are written in a perspicuous manner, and contain so much important information as to form a very valuable appendix to the work. A most copious and useful index, an appendage too often omitted in modern works, closes the volume.

It will not be expected that we should make extracts from every essay; but we shall endeavour to give some of the most interesting and useful parts of the work, for the information and instruction of our readers.

In the first essay, which is on the utility of a knowledge of chemistry to the arts, we are told that "formerly the professors of medicine were so ignorant of the nature of the salts, that no longer ago than the year 1765 there was a public dispute between the celebrated Margraff and M[illegible]achy, respecting the base of the *super-ta*[illegible]*h*, whether or no it was an alkali."

In one of the notes to the same essay we are told that " Boerhaave, who had a botanical garden of eight acres, and who was so intent upon stocking it with every exotic that he could procure, as once to have styled a present of a few American seeds '*munera auro cariora*,' gifts more precious than gold, was, notwithstanding, so captivated with chemistry, that he sometimes spent whole days and nights successively in the study and processes of the art."

The essay on TEMPERATURE is divided into two branches, viz. natural and artificial. The variety of climate in the different regions of the earth, the effects of caloric on animal and vegetable life, and the nature of its agency on combustible substances, are arranged under the first division of the subject; while that on *artificial* temperature contains a detail of a variety of expedients for procuring fire; for modifying the effects of heat and cold; for economizing fuel; and for improving many operations which have a considerable influence on the success of many of the manufactures of the country.

In speaking of the means which have been employed by different nations, and in different periods of the world, for producing fire, Mr. Parkes says, " In ancient times fire was always employed in the rites of religion; it consumed the burnt offerings of the Patriarchs; was kept continually burning in the Jewish tabernacle; was looked upon as the origin of life *, the soul of the world, the symbol of Deity; and considering it as the visible sign of an invisible Being, it has from time immemorial been actually worshipped by the Persians, and by some other Asiatic nations†.

" According to Pliny, fire was for a long time unknown to some of the ancient Egyptians; and when Eudoxus the astronomer showed it them, they were absolutely in raptures. The production of fire by collision, and the use of flint and steel ‡, were however known long before the time of Pliny."

In that part of the essay which relates to the subject of producing artificial cold, several chemical processes are related which can be conducted to better advantage at a temperature *below* freezing than at any other; and having remarked that " it might be worth while to inquire whether an ice-house might not be employed with advantage during the summer months in preserving meat," it is added that "from the salmon fisheries in Scotland and the north of England, the fish are sent to the metropolis, during the greater part of the season, packed with ice, in boxes about four feet long and eighteen inches deep. When packed, the ice, which is previously broken as small as bay-salt, is put over them and beaten down as hard as can be without bruising the salmon. In this manner they are kept perfectly fresh for two or three weeks."

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* Fire was so generally considered as the image of life, that *lighted* torches were usually placed in the hands of the newly married; and at their deaths *extinguished* torches were placed upon their tombs. Essai sur le Feu sacré, &c. 8vo. 1768.

† The sacred fire of the Vestal virgins, among the Romans, was beheld by them with little less than adoration. Numa built a temple to Vesta, the goddess of fire, which in after ages was rebuilt with great magnificence.

‡ The Laplanders [illegible] their contracts of marriage with the fire and flint; for fire with them is the [illegible] life, and the flint, say they, is eternal, for the treasure of fire within it never [illegible]

In treating of the advantages which may be derived from artificial cold, Mr. Parkes recommends the use of frigorific mixtures to freeze occasionally the brain and the eyes of those animals which are to undergo dissection by anatomical pupils. those parts not being easily dissected in any other way, and informs us that this method was first practised by the great Mr. Boyle. In the directions which are given for the use of saline mixtures to produce artificial cold, Mr. Parkes has very properly remarked that "it is of consequence to have the salts fresh crystallized, thoroughly dried, and then finely pulverized; that the mixtures be made rapidly, and in vessels as thin as can be procured."

An expedient related by Mr. Parkes, and which is adopted by people in northern regions for procuring water in winter, is curious. "During the winter at Hudson's Bay, the surface of the lakes and rivers is covered with ice of such great thickness, that no water can be procured without cutting through the ice with axes and wedges, which is a very laborious and tedious operation. As soon, therefore, as the surface of the water which has been laid open has acquired a thin plate of ice, the labourer heaps over it a quantity of snow, which, by being a bad conductor of heat, prevents the caloric of the water from passing upwards according to its natural tendency. Then, during the remainder of the winter, the inhabitants have only to remove a little of the snow, when occasion may require it, and they have water immediately."

A hint which Mr. Parkes gives in vol. i. p. 284, respecting the fuel proper for steam engine and other large iron boilers, deserves to be noted. In such cases "sulphurous coals should be avoided, as the sulphur which rises during combustion is apt to occasion a rapid decay of that part of the boiler which is exposed to the action of the fire. It produces a sulphuret of iron, which wastes away as fast as it is formed."

The last 100 pages of this essay are chiefly occupied in giving directions for building stills, furnaces, chimneys, &c. and in advice respecting the choice and management of fuel, all arising from the author's experience in his own manufactory; but we must confine our notice to a short extract relating to the management of reverberatory furnaces, at page 302 of this volume :—" In reverberatory furnaces for the decomposition of neutral salts and the manufacture of alkalies, ash-pit-doors are of no use, as it is never necessary to check the fire suddenly, as in other operations. On the contrary, I have found it advantageous to have the ash-pits as deep, and the chimneys as high, as possible, in order to occasion a strong and uninterrupted draught. I cannot suffer this opportunity to pass without remarking, that the proprietors of reverberatory furnaces should contrive to continue their processes night and day; or, if that be impracticable, they should rake up the fire and stop the passage to the chimney so as to prevent the furnace from cooling during the night. Furnaces thus worked will last six or seven times as long as those do which frequently stand idle. The contraction of the materials during the time of cooling, and the subsequent expansion, wear them out rapidly." But in our estimation the most important part of this essay is that where the author urges the necessity of attending to *temperature* in the conduct of manufacturing processes. A great number of cases in corroboration of this advice are adduced, which are well deserving of the perusal of all practical men: see page 341—382. The detail of the facts above alluded to is concluded in the following manner :—" I trust enough has now been offered to induce every manufacturer who may peruse these loose hints, to apply them to his own business, and to consider whether some of his own peculiar processes might not be very materially improved by a variation of the temperature at which they have usually been conducted. This is the chief object I had in view in composing this essay; and as all the principal observations are founded upon experience and the best information I could procure, I entertain some confidence that it will be acceptable, and prove highly useful—especially to the British manufacturer."

" The superior knowledge of our manufacturers has been the great source of our excellence as a commercial nation; but there is a danger, amid the growing intelligence of the age, of our losing this proud pre-eminence, unless some spirited individuals, in every class of society, aim at informing the public mind, and exciting that emulation among our respective artists, which is sure to stimulate their exertions and lead them on to perfection." These remarks are closed by some spirited national observations, which will be read with considerable interest.

The third essay in these volumes is entitled Specific Gravity; and as we do not recollect ever to have seen any treatise expressly on this subject, we deem it as important as any which the work contains. It is well calculated to instruct young people in all the different modes of ascertaining the specific gravity of gasses, fluids and solids. It contains some [illegible] for the calculation of specific gravities, and is accompanied with drawings of the different instruments which are usually employed for that purpose.

Essay IV. is on CALICO-PRINTING. This essay commences with an historical account of the various modes of staining and ornamenting linen garments from the earliest ages; describes some of the processes of calico-printing in India; and then the different methods which are pursued by the calico-printers of Great Britain and Ireland in the present day. The managements are however so various, and the processes so diversified, that an attempt to detail them here would be incompatible with our circumscribed limits. A few extracts must therefore suffice from this essay.

What Mr. Parkes observes respecting the use of the dung of the cow may be interesting to our readers, as it is new and curious. "When the pieces of calico have been properly stoved, they are passed," he says, "through water at various temperatures, with a little cow-dung mixed in it. The intention of the dung is to absorb and remove that portion of the mordant which is not actually combined with the cloth, and which otherwise might stain the white or unprinted parts.

"I suspect," continues he, "the dung of the cow is serviceable in another way besides that of cleansing*, though the printer may not be aware of the nature of its operation. It is acknowledged that madder, cochineal, and some other dyes, produce much better colours on *woollen* than on cotton cloths, owing to the former being of animal, and the latter of vegetable origin. I presume, therefore, that the dung imparts an *animal* matter † to the fibres of the cotton, and that this animal matter acts as an additional mordant, and thus more powerfully attracts the colouring particles of the dye, than the mordants alone would be capable of doing. If a piece of calico, prepared with acetate of alumine, be divided into two parts, and the superfluous mordant removed from one of them by cow dung and water, and from the other by water only, though both fluids were at the same temperature, it will be found, on passing the two portions through a decoction of weld or quercitron bark, that the yellow will be much more intense and bright in that which had been submitted to the action of the cow-dung."

The account of the advantages of cylinder printing is thus given by Mr. Parkes:—"These machines," says he, "have not only the excellence of printing more correctly than can possibly be done by means of the block, but the saving of time and labour which they afford is great indeed. A piece of calico which would take a man and a boy three hours to print with one colour, or six hours to finish with two colours, may by this means be done in three minutes, or three minutes and a half, and then much more completely than could even have been imagined before the introduction of this invention." There are many new and important hints thrown out in different parts of this essay, but we must refer our readers to the work itself for the particulars. Some copper-plate engravings of the new apparatus accompany this essay.

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* [illegible] calicoes by immersion in a dung-vessel, may appear to be a strange [illegible]; but as this is the technical language of the trade, no other could be employed with propriety.

† Berthollet, who analysed the dung of the cow, found in it a substance partaking of the [illegible]

The work before us, besides numerous known facts judiciously selected, contains so much new, curious, and useful information, detailed in a pleasing and popular manner, that we would willingly enlarge our extracts if we could find room: but our limits forbid this at present.

A Practical Treatise on propelling Vessels by Steam, &c. By ROBERTSON BUCHANAN, Civil Engineer; illustrated with sixteen engravings. 8vo. 204 pages.

The subject to which this volume is principally devoted, has for some time occupied much of the public attention. After a short introduction, Mr. Buchanan states the peculiarities of the Clyde navigation, that being the river on which the first attempt, deserving of notice, to navigate by steam was made in this country in the year 1812: this forms the First Part; and here the author has introduced a popular account and description of that inestimable invention, the Steam Engine.—Part II. contains descriptions of various modes which have been proposed or tried for propelling vessels. Parts III. and VIII. an account of steam navigation on other rivers in Great Britain and Ireland.—An account of the steam boats in America occupies Part IV.—Part V. is devoted to vessels in America propelled by means of machinery driven by from eight to thirteen horses or mules, working a gin on the deck.—Part VI. is dedicated to subjects relating to the theory and practice of Naval Architecture, as being intimately connected with steam navigation, and embraces § 1. The resistance of fluids: § 2. Experiments illustrative of the motions of resisted fluids: § 3. Experiments made under the direction of the Society for the Improvement of Naval Architecture: § 4. Of the forms best adapted for stability: § 5. Of the forms best adapted to prevent rolling and pitching: § 6. Of steering: § 7. Improvements in ship-building—Mr. Seppings's and Mr. Walters's described: § 8, § 9. are occupied with hints respecting timber, and the removal of imperfections in the common construction. Part VII. Miscellaneous observations.—An Appendix.

This work deserves commen[illegible]he subject of steam-[illegible] navigation has been treated in a popular manner, while the information communicated by the author is lucid and satisfactory. The plates are remarkably well drawn, and engraved in the first style.

Mr. Taylor, of the Architectural Library, Holborn, has just published the Fourth Volume of the Antiquities of Athens, &c. measured and delineated by James Stuart, F.R.S. and F.S.A., and Nicholas Revett, Painters and Architects; edited by Joseph Woods, Architect.

This volume contains 88 plates, besides 15 vignettes, engraved by the best artists, uniformly with the preceding volumes; together with historical and descriptive accounts of the several subjects; also a portrait of Mr. Revett, from a picture painted by himself, and engraved in the line manner by Isaac Taylor, and memoirs of the lives of the authors.

Messrs. Stuart and Revett being detained at Venice, in their way to Athens, made an excursion to Pola, where they passed six months in measuring the subjects, and in making the drawings, which are now submitted to the public; and which formed a part of their original scheme of publication.

The admiration with which these remains of antiquity have always been mentioned, no less than their intrinsic merits, render it desirable that they should be offered in complete detail to the public, which has by no means been the case in any of the works in which they have hitherto been noticed. The subjects are an amphitheatre, the temple of Rome and Augustus, and the arch of the Sergii.

The sketch-books of Messrs. Stuart and Revett have furnished several plates of curious fragments of ancient architecture and sculpture found in the Greek Islands, with views of Mount Parnassus and the Rock of Delphi.

The exquisite sculptures which adorned the temple of Minerva at Athens have ever been objects of the highest admiration, and are now become particularly interesting, from the circumstance of a large portion of them having arrived in this country. Of these beautiful specimens of ancient art there are 34 plates, from drawings by Mr. Pars, representing the entire west frieze of the cell, with some parts of the north and south sides, and several of the metopes of the exterior frieze. These, with those already published in the second volume of this work, exhibit all the sculpture which remained of the temple at the time (1751) Stuart and Revett were at Athens. Amongst these are five plates, showing the state of the sculpture in the pediments in the year 1683, when visited by the Marquis de Nointel, from copies of the original drawings in the King's library at Paris.

These valuable documents show the entire composition of the sculpture in the west front.

Dr. Adams is preparing for the press, Memoirs of the Life, Doctrines, and Opinions of the late John Hunter; founder of the Hunterian Museum at the College of Surgeons in London. These memoirs are carefully collected from authentic documents and anecdotes, and also from the writings, lectures, and conversations of the deceased.

A Journal of Science and the Arts, No. I. *Edited at the Royal Institution of Great Britain.* Published Quarterly.

In a former number we announced the expected appearance of this publication, edited by Mr. Brande. The first number was published at the beginning of April, and contains—1. A paper by Sir H. Davy on his safe-lamp, of which we have already laid a description before our readers.—2. Demonstrations of some of Dr. M. Stewart's General Theorems; and an account of some new properties of the Circle: by C. Babbage, Esq. F R.S.—3. On some phenomena attending the process of Solution, and on their application to the laws of Crystallization: by J. F. Daniell, Esq. F.R.S. and M.R.I.A. This is a very interesting paper, and the experiments of the author tend to support the ingenious theory of Dr. Wollaston respecting crystalline arrangement.

"If a mass of any moderately soluble salt be suspended in a vessel of water, we may shortly observe that it is not equally acted upon by the fluid. We shall perceive that it has been more dissolved toward the upper than the lower part, and the whole piece will assume, more or less, the form of a cone, with the apex at the surface of the liquid. The particles of water which are in immediate contact with the salt combine with a portion of it, and thus becoming specifically heavier than the remainder, sink to the bottom of the vessel; others succeed, and follow the same course. A layer of saturated solution is thus deposited, which increases in bulk as the process proceeds, protecting in its rise that part of the mass which is covered with it, from further action. The power of the solvent is therefore longer exercised upon the upper than the lower surfaces, producing, by its gradual decrease, the above-mentioned peculiarity of shape. This modification of solution by gravity is entirely counteracted by agitation; but if the process be carried on in a glass vessel, with some care, the current of descending liquid may be rendered perceptible to the eye.

"But there is a much more important circumstance attending this process, which it is the particular object of the present paper to illustrate and consider. Independent of the modification of form produced by the cause above described, the surface of a body is never equally acted upon by a solvent. Striæ or ridges may be detected in various places, and, indeed, generally cover the whole of its superficies, which prove, not only that the mechanical attraction of the solid has resisted chemical action, but that it has resisted it more in some directions than in others. The following experiments, which only require time and moderate attention, while they give determinate results, are explanatory, at once, of the cause and progress of the phenomena.

"If we immerse an amorphous mass of alum in water, and set it by in a place where it may remain undisturbed for a period of three or four weeks, at the expiration of that time we shall find that it has assumed the pyramidal form before described. Upon a further examination, we shall observe that the lower end of the mass presents the form of octohedrons and sections of octohedrons, as it were, carved or stamped upon its surface. These figures will be high in relief, and of various dimensions.—They will be most distinct at the lower extremity, becoming less so as they ascend, till at length they are totally obliterated.

"A continuation of the process, however, would evidently resolve the whole into similar figures, their cessation arising solely from that superior power of solution which subsists in the upper stratum of the liquid.

"These crystalline forms are produced when the water is partially saturated with the salt, and acting with diminished energy is nearly counterbalanced by its mechanical structure; and we are thus put in possession of the important fact, that this latter power does not merely act, as has been hitherto supposed, in the grosser forms of aggregation, but in the more complicated and delicate arrangements of crystalline polarity.

"This regular structure is developed both when we employ an amorphous mass and a regular crystal, proving that the ultimate arrangement of particles is the same in both; and that the like disposition exists, both when the slowness of approximation has bounded the solid with symmetric planes, and when the suddenness of the condensation has forced the aggregated molecules into a more contracted space.

"This new process of dissection admits of more extensive application than might at first be imagined, and we are thus furnished with a method of analysing crystalline arrangements, which promises to lead to important results. The geometrical figures produced by these means, are not less determinate when the process has been carefully conducted, than those which result from the common methods of crystallization; and they are the more instructive, inasmuch as we are presented in the same group with an extensive series of modifications, and decrements of the primitive form, which shew, by their relative position, and mutual connexion, the gradual steps by which one form passes into another."

Borax, crystals of sulphate of copper, sulphate of magnesia, submitted to cautious solution, also yielded distinct crystalline forms. Crystals of carbonate of lime, carbonate of barytes, a crystal of quartz, and a polished carnelian, submitted cautiously to the action of their proper solvents, gave similar indications, but rendered less perfect, in some instances, by extricated gas making paths for its escape along the surface of the crystal, and there preventing the regular action of the acid. Bismuth, nickel, antimony, and some other metals gave evidence also of regular crystallization.—The author, after detailing these experiments, proceeds to inquire "whether this new method of analysis may not be calculated to throw some light upon crystalline arrangements in general?" He then describes particularly the crystalline forms brought to view on the surface of the piece of alum, with a view to a proper theory which may agree with all the results.

"It is evident," he remarks, "that no general theory of crystallization can be applied to the cases under contemplation, which is not founded upon such a disposition of constituent particles as may furnish all the modifications of form before described, by the mere abstraction of certain individuals from a congeries, without altering the original relative position of those which remain. That is to say, supposing we adopt the hypothesis of the spherical form of the molecules, it will not be sufficient that a cube may be constructed by the superposition of four balls upon the top of four other balls [*], and an octohedron by placing four spheres in a square, with two others in the interstices between them (in which two combinations it is evident that the position of no three particles is alike); but the disposition of the cube must include that of the octohedron, and this latter must be obtainable by the equal abstraction of certain members of the former, without interfering with the quiescent state of the remainder."

He then assumes, for inquiry, the sphere as the form of the primitive particles, and proceeds to construct figures to illustrate their relative position, and to prove whether by the abstraction of certain rows or quantities of spheres from the mass, without disturbing those that are left, the mass or any portion of it can be made to exhibit the required forms. He makes a base of 36 balls, on these other balls to the number of 25 are placed in a [illegible]ne occupying the interstices formed on the surface of the 36.

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[*] See the construction of the cube as proposed by W. H. Wollaston, M. D. Sec. R. S. Phil. Trans. 181[illegible]

On these alternate layers of 36 and 25 are placed till the height equals the base, that is, a cube is formed—the atoms represented by the balls being kept in their place by attraction. He then shows by appropriate figures that all the crystals exhibited on the face of the alum "may be satisfactorily derived from a series of spheres maintaining that relative position which they must assume if endued with the power of mutual attraction." "The surfaces and lines of the solids produced, are in no instance interrupted, or broken, by a space equal to the diameter of one particle. Will any other geometrical solid furnish as simple and satisfactory a solution?" The cube assumed as the integrant particle is demonstrated to be defective.

"But there are many substances in nature resolvable, both by mechanical division and chemical solution, into regular solids, which, it is evident, cannot in any way be constructed of spherical particles. The rhomboids, for instance, of carbonate of lime, and the flattened octohedron produced by the action of water upon a four-sided prism of sulphate of magnesia. Is the theory calculated only to resolve the peculiarities of the former class; or may it be extended by similar observations so as to include crystalline arrangements of every description?

"The latter of the two substances just instanced, would seem at once to point to a flattening of the elementary sphere, as affording a solution of the problem, with respect to its individual properties; but how far may this idea be generalized? And are there any peculiarities in this class of bodies, which may direct us to this explanation of their nature?"

Mr. Daniell then shows that spheroids may be so arranged as to yield all the forms and modifications which are the subject of inquiry, and concludes his ingenious paper as follows:

"A singular confirmation of the spheroidical form of the ultimate particles of crystallized bodies, offers itself in the contemplation of a local arrangement which is common to crystals of every substance. If we suppose two nuclei to be formed in any solution, in such a manner that the axis of one shall run in a contrary direction to the axis of the other, each will of course attract a particular system of particles from the surrounding medium. Should the two, therefore, come in contact, a greater number will be collected at the point of junction than at any other, and they will therefore arrange themselves in the least possible space. Accordingly we find, that whenever a crystal is attached to another, in such a manner that their axes run in contrary directions, if we pull the two asunder, we shall invariably be presented with a regular hexagonal arrangement at the point of junction, whatever be the form of the crystal, the nature of the substance, or the direction in [illegible]her part it would be disposed to separate by mechanical force. This observation has been repeatedly verified upon carbonate of lime, selenite, fluor-spar, quartz, topaz, and other mineral bodies.

"The foregoing experiments and observations are offered in support of the ingenious theory of Dr. Wollaston, whose simple and satisfactory elucidation of the principles of crystalline arrangement has solved the difficulties, and remedied the inconsistencies of all previous explanations of the phenomena. Former hypotheses, however laborious in their construction, were defective, and unsatisfactory in the fundamental data of their arrangement, and were incompetent even to explain the solitary fact from which they originally emanated.

"This, however, is found to stand the test of experiment, as far as it is applicable from the nature of the subject; and another analogy is thus opened to the admirers of the simplicity and beautiful connexion of the order of the universe, who will recognise, in the invisible and scarcely imaginable atoms of a crystal, the same forms which in incomprehensible magnitude roll their majestic courses in the planetary system."

The other articles in the number before us are:—4. On a singular malformation of the human Heart; by N. L. Young, Esq. —5. Some account of the external changes which take place in the Surinam Frog (*Rana paradoxa* Linn.) from its earliest stages till it becomes a perfect animal; by W. M. Ireland, Esq.—6. An account of the physical properties of the Malambo Bark.—7. A new blow pipe.—This contrivance consists in employing a vessel containing compressed air, and furnished with a syringe to renew the supply.

8. On Aqua regia, or Nitro-muriatic Acid: By Sir H. Davy. —Strong nitrous acid, saturated with nitrous gas, and mixed with a saturated solution of muriatic acid gas, produces no other effect than might be expected from the action of nitrous acid of the same strength on an equal quantity of water: the acid so formed has no action on gold or platina. Equal volumes of muriatic acid gas and nitrous gas being mixed over mercury, and half a volume of oxygen being added, the condensation will only be what might be expected from the formation of nitrous acid gas; and when decomposed or absorbed by the mercury, the muriatic acid gas, unaltered, is found mixed with a portion of nitrous gas. "It appears then, that *nitrous* acid and muriatic acid gas have no action on each other." The mixture of *colourless nitric acid* and muriatic acid of commerce is *yellow*, and dissolves gold and platinum. If gently heated, it gives off pure chlorine, and becomes deeper coloured; if the heat be continued, chlorine still rises, but mixed with nitrous acid gas, which may be separated from it by a small quantity of [illegible] heated till no more

chlorine can be procured from it, it can no longer act on gold or platinum, and nothing rises from it but a mixture of nitrous acid and muriatic acid. "It appears then that nitro muriatic acid owes its peculiar properties to a mutual decomposition of the nitric and muriatic acid; and that water, chlorine, and nitrous acid gas are the results: and the attractions which produce these results appear to be the attraction of oxygen for hydrogen to form water, and that of nitrous acid gas for water." Aqua regia "does not oxidate gold and platina, but merely causes their combination with chlorine; and when it produces neutral salts, they are mixtures, and not chemical combinations of nitrates and compounds of chlorine."

9. On the freezing of Wine, and the Specific Gravity of Sulphuric Acid; by S. Parkes.—10. Observations on the application of Coal Gas to the purposes of Illumination; by Mr. Brande. —11. An anomalous case of Chemical Affinity, by R. Phillips, Esq.—12. Effects of a paralytic Stroke upon the powers of adjustment of the Eyes to near Distances, by Sir E. Home, Bart.—13. Review of Beudant's "Essai d'un Cours élémentaire et général des Sciences physiques."—14. Life and Writings of Hedwig. &c. &c. &c.

LXII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

March 28 and April 4. THE reading of Dr. Thos. Thomson's paper on Phosphoric Acid was continued. In the series of experiments which the author had undertaken to ascertain with precision the weight of an atom of phosphorus, he noticed several combinations of acids which had not hitherto attracted the attention of chemists, and to which he gave new names. But as he did not pursue his analysis to the extreme in every case, he reserved his detailed observations on some of them for a future paper, his chief object being to detect the combinations of phosphorus. Many of his conclusions are the result of calculation on the previously-known combinations of atoms of oxygen with different bodies, rather than of direct experiment. In some cases his experiments did not correspond with his pre-conceived theory of atomical combination, and then he readily abandoned the accuracy of the former to the supposed infallibility of the latter. Nevertheless he found, beyond doubt, that phosphorus combines with lime in six different proportions; and that as three of these combinations could not be multiples, they were in direct opposition to the [illegible] theory so pompously announced by Berzelius. Hence he concluded that the Swedish Professor has established his canons rather too hastily, and from data which, however they may correspond with the chemical phænomena of six or eight salts, are incompatible with the facts relative to nearly a hundred others. Dr. T. operated chiefly on bones, and the phosphat of lime which they contain: in operating he followed the process proposed by Eckberg in 1795, and preferred the deflagrating of phosphorus in oxygen, to every other mode of ascertaining their relative combinations. But his apparatus being small, his experiments were often made on the combustion of a single grain of phosphorus in oxygen gas.

April 25. A very short paper by Sir Everard Home was read, as an appendix to his remarks on the effects of certain medicines on the circulation of the blood, and in proof of his opinion that their efficacy is entirely owing to their action on the circulation diminishing the pulse, &c. To confirm this position, it was suggested to him to try the effects of his gout medicine thrown directly into the blood without the intermedium of the stomach. With this view 160 grains were injected into the veins of a dog, when the animal in a few minutes became convulsed, his pulse lowered, respiration difficult, had evacuations, and in five hours died. On opening the stomach it was found inflamed, and the whole appearances were exactly the same as if the poison had been taken into the stomach. Sir E. considers this a demonstration, as far as it is possible in such a case, of the truth of his theory.

GEOLOGICAL SOCIETY.

A Report by Dr. Granville, on a Memoir of M. Methuon, on the Mode in which *earthy* and *metallic* Crystals, not of a saline Nature, are formed, was read on the 16th Feb.

"Crystals, according to Mons. Methuon, are not the immediate consequence of undisturbed solution or fusion; but the produce of a peculiar decomposition of amorphous crystallizable masses, the particles of which arrange themselves, during decomposition, according to certain laws of attraction; the process being carried on in the dry way, and in the air."

About twelve years ago, while engaged in some mineral pursuits in Elba, his attention was directed to a block of *argillaceous schistus with pyrites*, which appeared to have been recently detached from a stratum of that substance, forming the basis of a large mass of *sandstone* projecting from one of the sides of the mountain. On examining it he found that several capillary crystals of alum from ½ to ¾ of an inch in length covered its superior surface. This as well as the lateral sides of the stone were in an evident state of decomposition more or less advanced, to the depth of 1¼ inch. There was some dust lying in the intervals between the crystals, which he blew off; and he observed that the wind had previously scattered some of it around the stone as it lay on the earth. Struck with this singular appearance, and almost instantaneously seizing the hint from nature, Mons. Methuon raised with sticks an appropriate shelter, under which he placed the mineral, and frequently paid a visit to the apparatus. The elongation of the crystals and the accompanying decomposition of the stone, became every day more visible, until, at the end of two months, the *former* were nearly double in size, and the *latter* had increased in proportion.

"This discovery of the alum," exclaims M. Methuon, "being formed in the air, and not in water, made a strong impression upon me, and I confess I could not forbear thinking there existed some analogy between this formation, and that of the *earthy* and *metallic* crystals not of a saline nature. It was evident, indeed, that the alum, in this case, did not exist in the rock, but was the immediate effect of its decomposition; that a portion of the sulphur from the *pyrites* passed into the acid state by means of its contact with the atmospheric air; and that this acid, combining with the argil, formed the crystals of alum."

He repeated his observations, multiplied his experiments, and instituted more particular inquiries in the island, which led, he says, to a discovery of recent crystals formed from a decomposition of the amorphous masses in which they were implanted. "Nay, at one time, he seems to have caught nature in the very act of forming crystals of quartz on a mass of *silico-calcareous* earth, from the surface of which M. Methuon had carefully removed all signs of pre-existing crystallization. The author marked the spot well, and left it. After a few weeks, some small points of rock-crystal made their appearance; by degrees the pyramidal summits were formed; and these were gradually followed by the prism; its mass diminishing in size, as the crystal became more and more diaphanous. At the end of three-and-twenty months, the period at which M. Methnon quitted the island, there were *six beautiful crystals of quartz*, from $\frac{2}{3}$ to $\frac{3}{4}$ of an inch in length, and $\frac{1}{3}$ of an inch in diameter; the silico-calcareous stone around them being excavated in the same proportion. But a fluid seemed, in this instance, to have had a part in the formation of these crystals; for, although the locality of the fossil producing them is described to have been beyond the reach of the waves, yet it is admitted that their spray, particularly in tempestuous weather, often bedewed its exposed surface.

"In a similar manner did M. M. obtain crystals of *yenite*, some specimens of which he has preserved to this moment."

Afterwards in Piedmont, "on removing some indistinct crystals of *alalite* and *garnet*, which he had discovered on breaking an amorphous mass of those substances, and on taking the proper precautions, consisting in throwing some loose earth and stones against the surface from which the crystals had been removed; after the lapse of six years, M. M. had the satisfaction of gather-a *second* and a *third* crop of new beautiful crystals, formed during that period, some of which were sent to the public institutions at Paris.

Having transported some of the shapeless mass of *alalite*, *garnet*, *green idocrase*, *pyroxene*, and amorphous *pyrites;* and formed with them an artificial mountain, which was placed on the chimney-piece in his room; after many days and weeks of anxiety, he had at last the pleasure of *seeing crystals of all these substances emerge from this heterogeneous mixture*. "The first," says the author, "which I had the satisfaction of seeing on my artificial mountain, were small prisms of *pyroxene*; next came the summits of crystals of *alalite*, then planes of *garnet*, after which those of *idocrase* and *peridot* followed in succession."

To produce *new crops* of crystal from cabinet specimens, was the next attempt. In July 1814 three specimens of idocrase, properly examined and described, were delivered to him from the School of Mines, and in November were submitted to the *crystallizing process*. In April 1815 new crystals were found on the surface of each of the specimens.

From a number of similar facts M. Methuon conceives "that the natural process of crystallization originally begins in a partial decomposition of the surface of a *crystallizable* fossil; that from certain spots of this surface, where it has first begun, the decomposition proceeds in straight and narrow lines to other similar spots which in their turn send forth similar lines, sometimes parallel to the former, at other times crossing each other; thus dividing, or, more commonly speaking, carving or engraving the surface of the fossil into several compartments, which become, by a continuance of the process of decomposition, as many distinct pieces, constituting the body of the crystal in its rough state; and lastly, that during this process the substances of a different nature, contained in the mineral, separate, and arrange themselves, in one or more parts of the same compartment, the fossil mass continuing to be solid and hard, but fragile and easy to be broken;" the author having often broken, between his fingers, some which had before withstood the strongest percussions.

M. Methuon maintains that he has proved: "1st, That crystals begin to form at their summit, edges, and solid angles.

2dly, That nature produces, by a direct process, all simple and compound crystals, without first *forming* a *nucleus* in the latter. 3dly, That the *matter*, serving to form the crystals, is in the state of a solid mass before, and continues in that same state during the whole process of crystallization. It may be called *crystallizable* matter. 4thly, That *crystallizable* matter is that which has filled, by infiltration, the chasms and clefts of mountains, and the cavities of rocks; which composes the veins, the stalactites, and the stalagmites; and, in general, all that which constitutes accidental formations found in *blocks, nodules*, &c. within large masses."

He recommends for his crystallogenous process, "a bed one inch thick, composed of loose earth, obtained from the decomposition of the stone in which the crystallizable matter is found, having an elevated *brim* of the same material round it, one-third of an inch in height. Some balls made of the same earth are disposed here and there on this bed, on which are placed various pieces of solid *crystallizable* matter, formerly known under the name of *crystalline matrix*. On these pieces other balls are properly disposed, serving to support some more specimens of crystallizable matter, so arranged as not to touch each other. The whole of it is then made as solid as possible, by the addition of other large and small balls, introduced wherever any space exists; and lastly, the apparatus is surrounded by a wall of bricks laid singly on each other, without any mortar, and in a way to admit a free circulation of air.

"Every two or three days the whole apparatus is watered, so as to keep it in a state of constant humidity, and no more. A degree of temperature is maintained equal to the internal temperature of the earth; and the apparatus is examined every fortnight or three weeks; when, if necessary, the pieces may be carefully washed and replaced, taking care to arrange them so, that the balls which before were under, may now be placed above. After a certain lapse of time, the crystallizable matter is found to present distinct and beautiful crystal of the substances employed."

ROYAL SOCIETY OF EDINBURGH.

Papers on the following subjects have been read at this Society since the commencement of the present session:

On the Optical Properties of Fluate of Lime and Muriate of Soda [illegible] Dr. Brewster. The author has ascertained that this class of crystals possesses the property of double refraction. Large masses of muriate of soda, fluate of lime, alum, and diamond not only possess this property, but do so in a manner different from all the other crystals of the mineral kingdom; combining in the same specimen the structure of both the classes of doubly refracting crystals. In one part of their mass they have the same structure as calcareous spar, and crystals of that class; in another the structure of sulphate of lime, and crystals of that class; and in some parts they do not exhibit double refraction.

Experiments made at Woburn Abbey for determining the Proportion between the Load and Draught of Horses in Waggons. The instrument employed was contrived by Mr. Salmon of Woburn. One general result was, that in a four-wheeled waggon of the ordinary construction, on a good road and on a horizontal plane, the draught is between a 25th and a 30th of the load. With a load of one ton the draught is between 75 and 80 pounds. Several other results were deduced.

Account of a Chromatic Thermometer, by Dr. Brewster. This instrument is founded on a newly discovered action of heat upon glass. Glass is thrown into a *transient* state of crystallization during the propagation of heat through its mass. When in this state it acts on polarized light like crystallized bodies, and produces various orders of colours in different parts of the glass-plate. The number of fringes increases, or the tints rise in Newton's scale, as the temperature of the source of heat is increased; so that the difference between the temperature of the glass and that of the source of heat, is measured by the number of the fringes, or the nature of the tints which are developed. As every tint in the scale of colours has an accurate numerical value, differences of temperature may be measured with the utmost correctness, from the lowest temperature up to those at which glass begins to lose its solidity.

The heat of the hand applied to a single plate of glass *three-tenths* of an inch thick, produces a perceptible effect in crystallizing the plate; so that, if *ten* plates were employed, a difference of temperature, equal to one-tenth of that which was applied to the single plate, will be distinctly appreciable.

Extracts from an unpublished Memoir of Laplace on the Application of the Calculation of Probabilities to Physics. The extracts referred particularly to the figure of the earth as deduced from the vibrations of pendulums. He finds from thirty-seven of the best experiments on the length of the seconds pendulum in different latitudes, that the increase of gravity from the equator to the poles, follows the law which theory points out as the most simple; and concludes that the density of the earth must augment regularly from the surface to the centre; and hence he infers the original fluidity of the whole—a state, he adds, which nothing but excessive heat could produce. [This fluidity may be allowed without admitting the cause he has assigned for it.] From his formula Mr. Playfair finds the length of the seconds' pendulum for London to be 39·13009 English inches. The bill now before parliament for equalizing weights and measures states the length to be 39·13047.

An Account of the Sleeping Woman of Dunninald, near Montrose; by the Rev. James Brewster. Her first sleeping fit lasted from the 27th to the 30th of June 1815. Next morning she again fell into a sleep which lasted seven days— without motion, food, or evacuation. At the end of this time, by moving her hand and pointing to her mouth, it was understood she wanted food, which was given to her; but she remained in her lethargic state till the 8th of August,—six weeks in all, without appearing to be awake, except on the 30th of June Her pulse for the first two weeks was about 50; the third week 60; and previous to recovery it was at 70 to 72. Though extremely reduced, she gained strength so rapidly that before the end of August she worked regularly at the harvest. This case is well authenticated.

In a paper on Barometers, by Mr. Playfair, iron tubes are recommended for barometrical observations in mountainous or remote countries. The tubes being truly bored, and of the proper length, the mercury is to be poured into them at the place of observation. A finger being then placed on the orifice, the tube inverted in a cup of mercury, and being again stopped with the finger (after all oscillation has ceased) and withdrawing the cup, the quantity in the tube will give the result. The quantity may be measured by means of a graduated float.

Papers on the following subjects have also been read. On an Aerolite which fell near Bombay on the 5th of November 1811. On Means for Lighting Coal Mines. On the Education of Mitchell, the blind and deaf Lad. On Analyses of Sea-water. An Account of an Animal found in Horses' Eye in India. Experiments on Light; also a Paper on the probable Existence of a new Species of Rays in the Solar Spectrum, apparently produced by the Collision of the Particles of Light when emitted from the Sun's Surface; by Dr. Brewster. And an Account of some Veins of Greenstone which traverse the Granite of Sabie Mountain.

WERNERIAN NATURAL HISTORY SOCIETY OF EDINBURGH.

On Saturday, the 13th of April, Mr. Thomas Forster read a paper to the Wernerian Society of Natural History of Edinburgh, on the generic varieties in the forms of the brain and crania of animals, and the possibility of distinguishing the genus and sex of animals by the figure of the skull. Mr. F. illustrated his paper by about fifteen drawings of crania of different animals made from nature, [illegible] direction, by Mr. Lizars of Edinburgh.

ROYAL

ROYAL INSTITUTE OF FRANCE.

Analysis of the Labours of the Class of Mathematical and Physical Sciences for the Year 1815. *By M.* CUVIER.

CHEMISTRY.

We have during the last two years spoken of those acids without oxygen, or, as they are now called, *hydracids*, which have made such a considerable breach in the imposing edifice of the chemical theory of Lavoisier. The labours of M. Gay-Lussac have this year proved that there is one more to add to this class: that which M. De Morveau had called prussic acid, because it enters into the composition of Prussian blue, and because, its radical not being known, it was not possible then to derive from that source its denomination.

The experiments of Margraaf, Bergman, and Scheele [illegible] not admit of a doubt that in Prussian blue the iron was [illegible] with a substance which performed the part of an acid. M. Berthollet had suspected however for a long time that oxygen did not enter into its composition, but merely carbon, azote and hydrogen; and this suspicion has been changed into a certainty by Gay-Lussac.

On decomposing, with the precautions which he points out, the prussiate of mercury by the hydrochloric (otherwise muriatic) acid, he obtained pure prussic acid; and we have already spoken in one of our preceding reports of the singular properties which he found in it in this state, and chiefly of its extreme volatility. On afterwards burning the vapour of this acid by oxygen and the electric spark, he obtained determinate quantities of water, carbonic acid and azote. The oxygen consumed in the production of the two first of these substances is wanting; and it follows from this conclusion, that one volume of vapour of prussic acid results from the combination and concentration of one volume of vapour of carbon, half a volume of azote, and half a volume of hydrogen; or, by expressing these volumes by weights, according to the density of each of these vapours, 100 parts of acid contain 44·39 carbon
51·71 azote
3·90 hydrogen.

Thus the prussic acid contains more azote and less hydrogen than the other animal substances, from which it is particularly distinguished by the total absence of oxygen.

This is the first hydracid known, the radical of which is decomposable; and this radical M. Gay-Lussac also succeeded in obtaining freed from its hydrogen. Not being able to preserve this epithet of *prussic*, which belongs only to [illegible], he has given it the appellation of cyanogene (that is [illegible] producing *blue*). The prussic acid will therefore take [illegible] denomination of *hydrocyanic*; its combinations with the bases, that of *hydrocyanates*, and the combinations of its radical, that of *cyanogen*.

We would fain give an account of the numerous and delicate experiments by which M. Gay-Lussac referred to the one or the other of these classes the various products resulting from the action of the prussic acid on bodies, and all the properties which he has brought to light; but our limits do not admit of it. Suffice it to say, that Prussian blue in particular seems to him to be rather a cyanure of iron which has retained water, than a *hydrocyanat*, or, as it was formerly called, a *prussiate*.

This cyanogene, considered by itself, presented some remarkable properties: it is a permanent elastic fluid, the density of which to [illegible] the air is as 1. 8064 to 1, of a peculiar pungent smell, [illegible] sharp taste to water, and burning with a purple flame. Water absorbs four times its volume, and alcohol 23: its direct analysis gave the same result with that of the hydrocyanic acid, *i.e.* one volume of vapour of carbon for half a volume of azote.

[To be continued.]

LXIII. *Intelligence and Miscellaneous Articles.*

COMMUNICATIONS RESPECTING SIR H. DAVY'S SAFE-LAMP.

HAVING learnt by the Newcastle newspapers that a deputation from the coal-owners of the rivers Tyne and Wear, &c. had waited on Sir Humphry Davy, on his return from Scotland through Newcastle, in the end of March, to express to him their opinion of the merits of his invention for guarding the lives of miners against the effects of fire-damp explosions, and to thank him for the benefits thus conferred on humanity; we were anxious to be enabled to give all the publicity in our power to such an unquestionable testimony, as being the most satisfactory reply to the animadversions which appeared in another periodical work on the observations which we have before made on this most important discovery. We could not for obvious reasons request Sir Humphry to furnish us with the documents wanted. We applied to the chairman of the meeting alluded to, and our request met with that prompt attention which we anticipated. The facts are briefly these: A deputation from the coal-[illegible] addressed the following note

"To Sir *H. Davy, &c. &c. &c.*

"Messrs. [illegible] Brown, Potts, Watson, Cronden, Waldie, Jas. [illegible]ddle, forming a deputation from the coal-[illegible] Tyne and Wear, and the ports of Hartley and Blyth, request the honour of paying their respects [illegible] H. Davy, and of presenting to him a letter contai[illegible] expression of the thanks of the coal-owners."

The following is the letter which the said deputation delivered into the hand of Sir Humphry:

"*To Sir Humphry Davy, LL.D. &c.*

"Newcastle, March 25, 1816.

"Sir,—As chairman of the general meeting of proprietors of coal-mines upon the rivers Tyne and Wear, held at the Assembly Rooms at Newcastle on the 18th inst., I was requested to express to you their united thanks and approbation for the great and important discovery of your safety-lamp for exploring mines charged with inflammable gas, which they [illegible] admirably calculated to obviate those dreadful calamit[illegible] lamentable destruction of human lives, which of late [illegible]quently occurred in the mines of this country.

"They are most powerfully impressed with admiration and gratitude toward the splendid talents and brilliant acquirements that have achieved so momentous and important a discovery, unparalleled in the history of mining, and not surpassed by any discovery of the present day; and they hope that whilst the tribute of applause and glory is showered down upon those who invent the weapons of destruction, this great and unrivaled discovery for preserving the lives of our fellow-creatures will be rewarded by some mark of national distinction and honour.

"I am, sir,
"Your most obedient humble servant,
"GEORGE WALDIE, Chairman."

As, on a question of the kind before us, no testimony can be of equal weight with that of the people most directly interested in the prosperity and safety of the mines,—in addition to the foregoing, and to what we have given in another page of our present number respecting the coal-mines at Whitehaven, we subjoin an

Extract of a Letter from John Peile, Esq. on the Use of the Wire Gauze Safe-Lamp.

"Col[illegible] Whitehaven, Feb. 21, 1816.

"It gives us great pleasure to add our confirmation on the safety of Sir H. Davy's safe-lamp, and to express our confidence in the security of this simple yet curious-invented instrument. We this day put its efficacy to the test in the most dangerous places we have in William pit, and in each [illegible] the result corresponded with the description publ[illegible] experiment was in Russia stem-drift, with some [illegible] fire-damp from a stratum of stone. In approaching the confines of the foul air, the flame of the wick increased in bulk, and by progressively advancing, the wire cylinder first became filled with flame from the explosive mixture, and at last the whole extinguished without producing the least flame on the exterior of the wire. These experiments were frequently repeated with the same effect. We then proceeded to the Trial Inbanks, where we have for many months been working with steel-mills, not daring to introduce a candle on account of the quantity of fire-damp generated. In these places the same safety and effects were found as in Russia drift, to the great astonishment of the workmen employed.

"The simplicity of the lamp is beyond description, and, except [illegible] the repeated proof of its security, to look at, it appears incredible.

"In all places where danger is the least suspected, there can be no doubt the lamp will be absolute security if properly applied, and in a little time it will become in general use. The light produced from the lamp (trimmed with spermaceti oil) was quite sufficient for the ordinary purposes of working. The thanks of miners must ever be given to Sir Humphry Davy for this momentous discovery. I have the honour to be, &c.

"JOHN PEILE."

We have great pleasure in stating that the coal-trade has liberally presented 100 guineas to Mr. Stevenson, of Killingworth Colliery, for his ingenious lamp described in a preceding number of the Philosophical Magazine, which, though superseded by Sir Humphry Davy's more perfect invention, not only evinced great ingenuity, but promised much comparative safety to the miners.

The public have been amused within the last few weeks, by a boy with extraordinary calculating powers, who is exhibited by his friends in the Great Room at Spring Gardens: Perhaps the following short account of the calculating phænomenon of England, as he is called, will be a satisfaction to our readers.

George Bidder was born in a cottage at Morton Hampstead, twelve miles from Exeter, Devonshire, on the 14th of June 1806. His father William Bidder, a hard-working mason, principally occupied in making the stone fences with which that country abounds, has seven children, four boys and three girls: the boys assist the father, girls at service; George, the youngest but two, whose time was employed as country children's are, went to a three-halfpence a-week school till seven years old, when the first [illegible] his [illegible] extraordinary abilities was in reckoning the [illegible] in a [illegible] shoes, and by degrees doubling them from a farthing [illegible]; this brought on other questions, when his ready replies induced his father to make a tour to the principal towns: Bristol, Liverpool, Birmingham, Bath, Cambridge, &c. &c. where he gave universal satisfaction. In London he appeared before the Dukes of Kent and Sussex, Lord Stanhope, Sir Joseph Banks, and the principal nobility and gentry. Her Majesty having signified her commands, he appeared before her and the three Princesses at Windsor, and answered the questions proposed to him by the Bishop of Salisbury, without the least agitation or hesitation, so quickly and correctly, as highly to please Her Majesty, who made him a handsome present. He continues to improve in his calculations, and solves very difficult questions in a manner to astonish and delight the company. He is just now learning to write; figures he cannot make, nor is it intended he should be taught yet—Lord Stanhope, who has much noticed him, advising his friends against it, as fearing it may in some measure interfere with that intuitive faculty he at present possesses; and certainly the knowledge of figures could not make him more ready than he now is.

The following, among many others, are questions he has correctly answered:

1. Suppose a cistern capable of containing 170 gallons, to receive from one cock 54 gallons, and at the same time to lose by leakage 30 gallons in one minute; In what time will the said cistern be full?

2. How many drops are there in a pipe of wine, supposing each cubic inch to contain 4685 drops, each gallon 231 inches, and 126 gallons in a pipe?

3. How many times will a wheel of 8 feet 3 inches in circumference revolve in going 999 miles?

4. Suppose the national debt to be 802,032,000*l.*; If I pay 147,000*l.* a day, how long shall I be in paying it off?

5. What is the square root of 88,115,769?

6. If a person is 14 years old, and walks 14 miles each day, reckoning 365 days to the year; How many inches has he walked?

7. If I purchase nine marbles for one halfpenny; How many can I purchase at the same rate for 1075*l.* 10*s.* 2½*d.*?

8. How many groats are there in 498,265,316 farthings?

9. Suppose St. Paul's was 20 years building, and 500 people daily employed, and each consumed ¾ lb. of meat per day; How much was consumed in the 20 years?

10. Suppose a circular reservoir to contain 10,669 hogsheads at 6 feet in depth; What will it contain if made 10¼ inches deeper, and in what time would the whole be full from a spring producing 1 hogshead per minute?

11. If a man was to fall from the sun 80,000,000 miles, at a mile per minute; How long would he be falling?

12. In the cube of 36; How many times 15228?

STEAM ENGINES IN CORNWALL.

It appears by Messrs. Lean's Report for March 1816, that the average work of thirty-five engines was 19,720,466 pounds of water lifted one foot high with each bushel of coals consumed; that Woolf's engine at Wheal Var, during that month, lifted 48,432,702 pounds, and his engine at Wheal Abraham 49,966,698 pounds one foot high with each bushel of coals.

We understand that the fine Collection of Minerals, which belonged to the late Rev. Mr. Hennah, of St. Austell in Cornwall, will be sold by Mr. King early in June next.

Theatre of Anatomy, Medicine, &c. Blenheim Street, Great Marlborough Street.—The Summer Course of Lectures at this School will begin on Monday, June 3, 1816.

Anatomy, Physiology, and Surgery, by Mr. Brookes daily at Seven in the Morning. Dissections as usual.

Chemistry and Materia Medica, daily at Eight in the Morning.

Theory and Practice of Physic at Nine, with Examinations by Dr. Ager, Fellow of the Royal College of Physicians, &c.

Three Courses are given every year, each occupying nearly four months. Further particulars may be known from Mr. Brookes, at the Theatre; or from Dr. Ager, 69 Margaret Street, Cavendish Square.

Dr. Clutterbuck will begin his Summer Course of Lectures on the Theory and Practice of Physic, Materia Medica, and Chemistry, early in June.

LIST OF PATENTS FOR NEW INVENTIONS.

To John Sorby the younger, of Sheffield, in the county of York, edge-tool maker, for a method of making an auger for the use of shipwrights, millwrights, carpenters, and other artificers, upon a new and improved construction.—23d March 1816.—2 months.

To William Macnamara, of East Smithfield, plate-glass manufacturer, in consequence of a communication made to him by a certain foreigner residing abroad, for his method or methods of manufacturing glass.—23d March.—6 months.

To Uriah Haddock, of the parish of Holloway, chemist, for a new species of paint-colour and cement for painting and colouring and preserving the interior and exterior of houses, ships, and other things.—23d March.—6 months.

To William Lewis, of Brimscomb, Gloucestershire, dyer, for his new machine for fulling woollen or other cloths that require such process.—5th April.—6 months.

To Joseph Turner, of Layton, in the county of York, for his improved rotary engine, and application thereof, with or without other machinery, to useful purposes.—8th April.—2 months.

To John Woodhouse, of Bromsgrove in the county of Worcester, for his method of forming the ground for roads and pavements, and also for paving and repairing old pavements and roads.—9th April.—6 months.

To William Atkinson, of Bentinck-street in the parish of St. Mary-le-bone, for his new or improved method or methods of forming blocks with bricks and cement in the form of Ashlar-stone for building, so as to have the appearance of stone.—9th April.—6 months.

To William Stenson, of Coleford, Gloucestershire, for his improved engine to be worked by steam or any other power.—9th April.—6 months.

To William Lapalle, of the city of Bristol, for his method or contrivance for an improvement in the construction of a gig, and of cards, so called, in the clothing and other manufactories, or other machines or instruments used and employed in such manufactories for the same or similar purposes.—23d April.—2 months.

Meteorological Observations made in Scotland, principally at Edinburgh, from March 18th to April 13th 1816.

March 18.—(At Sterling) Showers with wind from the northward.

March 19.—Sky chiefly obscure with less wind, fair calm evening. Wind again at night. Dark night.

March 20.—Fair day with *cumuli*, &c. and little wind. Dark night

March 21.—Showery weather returned, with fair intervals; temperature moderate.

March 22.—Obscure and windy.

March 23.—Fair day, and sky chiefly clear with *cumulus*, &c.

March 24.—Obscure and calm, followed by whistling wind sweeping dust along.

March 25—29*.—Obscure in general, but dry and with wind in gales.

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* On the evening of the 27th being on the hills above Edinburgh about 4½ o'clock, I noticed a great clearness in the windward, i. e. east; while the town to the westward and southward was involved in thick mist. This must

March 30, 31.—Fair with gales blowing along clouds of dust; wanecloud and stackencloud, &c. but no distinct appearances of rain.

April 1.—Obscure but fair day; with gentle gales.

April 2.—Fair gray day, cloudy with gales at night from the southward. The dust became troublesome, and there was a whistling noise in the wind which often forebodes rain; with a ring or *corona* about the moon.

April 3, 4, 5.—Cloudy and in general obscure, with fine intervals and light gales of wind. I did not pay particular attention to the weather these three days, being otherwise engaged.

April 6.—Towards evening gentle showers of rain. The atmosphere cold and winterlike. The *crocus vernalis* in flower in an inclosed piece of ground in the middle of Edinburgh Old Town.

April 7.—Rainy morning. It cleared toward noon, but showers returned again from the northward at 2 p. m.

April 8.—Showery weather, with snow and sleet and cold. I was very sensible of the difference between the weather here and that in England, particularly in the southern parts at this time of the year.

April 9.—Fine spring-like day, stackenclouds or *cumuli*, with wanecloud in the afternoon. The distances were clear to windward; but the crows about the seashore near Leith and Portobello flew about, alighting frequently near the water's edge, and uttering a hoarse and frequent cry—an indication of rain noticed by Aratus; and which was verified on the morrow.

April 10.—Obscure and rainy day; it held up for a very short time now and then. Air cold, and windy from N.W.

April 11.—Clouds and a great deal of small rain. Fine at night.

April 12.—Warmer in the morning, and the clouds lofty with breaks, through which the sun came out. Cold wind again at night.

April 13.—Clear morning, showers of snow succeeded, but it did not lie on the ground. Towards evening a strong N.W. wind blew, and it became clear and very cold, with a frosty night.

10, College-Street, Edinburgh,
April, 14th, 1816.

THOMAS FORSTER.

must have arisen partly from the smoke from the chimneys of the houses, but not entirely, as I noticed that Leith was partially involved in mistiness, and partially clear; and that the misty places changed, so that the fallcloud causing the misty appearance appeared to travel along with the wind. I have observed that Edinburgh is more misty in proportion to the wind blowing than any other place I have yet seen.

METEOROLOGICAL JOURNAL KEPT AT BOSTON, LINCOLNSHIRE.

[The time of observation, unless otherwise stated, is at 1 P.M.]

	Age of the Moon.	Thermometer.	Barometer.	State of the Weather.	Modification of the Clouds.
1816.					
March 15		50 0	29·30	fair	strong gale from the westward
16		42 5	29 95	do	
17		43·0	29·92	do	
18		46·0	29 60	rain	
19		45·0	29·57	fair	
20		42·0	30·15	do	
21		48·0	30·10	do	
22		50·0	30·25	rain	
23		47·0	30·40	fair	
24		44·5	30·50	do	
25		40·0	30 30	do	
26		41·5	30·35	do	
27		45 0	30·40	snow & rain	
28	new	44·5	30 30	fair	
29		42·0	30·28	do	
30		45·0	30·40	do	
31				do	
April 1		49·0	30·15	very fine	severe frosts at night
2		47·0	30 0[illegible]	do	
3		42·5	30 10	do	
4		42·0	30·15	do	
5		47·0	30·10	do	
6		50·0	29·60	fair	
7		46·0	29·15	stormy, with hail, rain, and snow	
8		45·0	29·25	fair	
9		45·0	29·45	do	
10		54·0	29·45	cloudy, rain in the evening	
11		49·0	29·75	do. and showery	
12	full	45·0	29·80	most violent	rain all this day and the following night. succeeded by intense frost
13		34·0	29·90	much snow and hail	
14		40·0	29·72	snow storms	
15		43·0	29·75	slight showers of snow and sleet	

METEOROLOGICAL TABLE,

By Mr. Cary, of the Strand,

For April 1816.

Days of Month.	Thermometer.			Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
	8 o'Clock, Morning.	Noon.	11 o'clock Night.			
March 27	38	40	37	30·20	16	Cloudy
28	35	40	35	·13	22	Fair
29	36	42	37	·15	27	Fair
30	37	44	38	·20	15	Cloudy
31	38	46	36	·20	29	Fair
April 1	37	49	37	·04	32	Fair
2	37	47	38	29·80	30	Fair
3	36	44	35	·82	30	Fair
4	35	52	35	·91	37	Fair
5	32	55	41	·87	30	Fair
6	40	55	41	·61	29	Fair
7	40	40	40	·10	20	Storms of hail
8	40	47	38	·15	24	Cloudy
9	38	43	40	·20	20	Sleet showers
10	40	51	42	·28	16	Cloudy
11	42	47	42	·50	0	Rain
12	41	47	40	·70	18	Cloudy
13	37	37	30	·77	16	Sleet
14	30	35	30	·51	0	Snow
15	36	46	40	·64	15	Fair
16	40	47	41	·50	14	Showery
17	40	52	41	·47	16	Showery
18	45	50	42	·52	24	Fair
19	45	55	40	·80	36	Fair
20	43	55	42	·99	38	Fair
21	41	49	44	·75	0	Rain
22	46	55	46	.69	0	Rain
23	55	66	51	·76	27	Fair
24	50	66	52	·80	46	Fair
25	55	66	48	30·03	47	Fair
26	47	65	49	·05	56	Fair

N.B. The Barometer's height is taken at one o'clock.

LXIV. *On Aërial Navigation.* *By Sir* GEORGE CAYLEY, *Bart. F.R.S., &c.*

To Mr. Tilloch.

SIR,—I AM glad to see the subject of aërial navigation noticed by Mr. Lovel Edgeworth, and likewise by a gentleman under the signature of T. H. in your Magazine for March last. Mr. William Bland has likewise written an ingenious paper upon this subject in the Monthly Magazine for March. I wish to bring all those who interest themselves in this invention, to act in concert towards its completion, rather than to be jealous of each other respecting their own share of credit as inventors: indeed, unless we can realize our object, very little credit will be due to these speculations. Mr. Edgeworth, who upon good grounds puts in so early a claim to a knowledge of the principle of steering balloons by the inclined plane, has quite misconceived the nature of the principle upon which my former papers upon the subject of aërial navigation were founded, when he says that "Sir George Cayley has frequently proposed to impel flying bodies by letting them descend obliquely through the air, and forcing them in a contrary obliquity against the air by *impelling them upwards.*" My plan rested upon the following fact: that if a plane of any given magnitude, say 100 square feet, were placed so as to make any small angle with an horizontal line, suppose an inclination of one to ten: then, if it were propelled forward in the horizontal path, like a bird in the act of skimming, by a force of ten pounds, till the horizontal resistance of the air equalled this pressure, the plane would have power to sustain one hundred pounds weight during its progress. I proposed to create this slight horizontal pressure by the power of a light first mover, several of which were there alluded to, particularly one where the combustion of oil of tar was made use of as the moving power. This engine had been exhibited in a working state to Mr. Rennie, Mr. Cartwright, and other gentlemen capable of appreciating its powers; and it appeared from the minutes of Mr. Chapman, civil engineer of Newcastle, that eighty drops raised eight hundred weight the height of twenty-two inches; hence a horse-power may consume from ten to twelve pounds per hour. The expense of power, however, with this engine being much greater than with the steam-engine, the patentee never proceeded further than the original experiments. From this statement it is plain that the tacking operation alluded to by Mr. Edgeworth,—by "*impelling them upwards,*" and then letting gravity operate in the descent;—was not any part of my plan, although, being acquainted with the principle of oblique forces, and not having a proper first mover, I was obliged to try the experiments as to steadiness and steerage by merely allowing the vessel to sail from the top of a hill; and it was to this circumstance that I alluded in my observations upon Mr. Evans's paper, the descending half of the movement that gentleman proposed being perfectly similar to, and therefore corroborated by my experiments.

I have related this matter more minutely, because Mr. Edgeworth's statement rather gave me a claim to the principle of steering balloons by means of the inclined plane, which I do not possess. Indeed I hold it perfectly in my recollection, (although I cannot state either the name of the party or the publication in which I read the account,) that an ingenious young man several years ago had tried some successful experiments on the steerage of balloons by the inclined plane, but had died before he completed his invention.

Should this subject gain importance by experimental success, this, and no doubt many other claims of various kinds, will be attempted to be placed over the heads of those gentlemen, who now will, I hope, have the more substantial credit of realizing and perfecting the invention for the uses of mankind. For my own part, I shall sue for no share in the scramble respecting Montgolfier balloons, but that of braving the risible muscles of my friends in substituting *acres* for *yards* of cloth in their structure, unless indeed it be for the addition of a *chimney*, a most common, natural, and in this case particularly useful appendage to a fire. I shall however be extremely gratified in being able to claim the credit of standing on the same list with Mr. Edgeworth as a subscriber towards completing this noble art;—with this view I request Mr. Tilloch to be kind enough to permit a list of subscriptions to remain at his house till a few names be collected and a committee appointed*;—no one to be called upon for his subscription money till such a sum be subscribed for as will be sufficient to try the first experiment proposed by the committee.

In my last paper it was stated that I had made some calculations relative to the quantity of fuel that would be consumed in propelling the Montgolfier balloon there described, at a velocity of twenty miles per hour, by the steam-engine. According to the returns made of the Cornwall engines, Mr. Woolf's engine raises about fifty millions of pounds, one foot high, with a bushel or eighty-four pounds of coal. Hence I estimate that twenty pounds of coal per mile would be consumed in this operation,

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* I can have no objection to keep a list of the names of such gentlemen as may wish to contribute to an experiment of the kind recommended.—A. T.

ration, and therefore the expense would not be considerable.— But as no more than 6800 pounds, the unconsumed power of this balloon, can be allowed for the weight of the engine, its water and fuel, besides that of an extensive surface for wafting, it appears upon estimating the weight of these things*, that this balloon, extensive as it is, is, as I before said, only the long-boat of its species, and not quite large enough to take advantage of the saving power of the steam-engine at so great a velocity as twenty miles per hour: though it will answer the purpose of a first experiment at fifteen miles per hour with a proportionate saving of fuel.

Upon larger constructions, however, it will appear that aërial navigation will be performed much cheaper in a horizontal path by the steam-engine, than upon the ascending and descending plan with the inclined plane. Several years ago I made many estimates of the application of first movers to these long balloons; but I always found that the enormous size required to be successfully driven by them, placed the proper scale of experiments quite out of the reach of any individual, and unfortunately of such magnitude as to render the public of that day, if appealed to, more incredulous than ever upon the subject. My own experiments were therefore confined to the inclined plane, which offers a good result upon a smaller construction. The introduction of the steam-boat, together with "the steady march of the human mind" during so many years of unexampled scientific discovery, attended by practicable results in the rapid improvement of almost every art, will now enable me to state my ideas upon this subject, without stepping beyond the limits of many an inquiring mind. My former paper showed that the Montgolfier balloon there described was only the long-boat of this class of vessels; yet I felt obliged to pave the way for the introduction of so huge an infant obtruded upon the civilized world, by leading the unprepared mind from the contemplation of a hundred-gun ship of ninety yards in length, to a balloon of a hundred; and although in the course of this paper I shall be obliged to point out objects in reserve upon a larger scale, yet as a matter of experiment, I have nothing to add to the *bulk* of the balloon already described. It would be a great advantage to that vessel if the greater portion of its long chimney, from about twenty feet above the fire, were made of flexible materials well coated with the mixture of brick-dust, &c. with which the firework-makers protect their materials:—this would allow of

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* Coals for one hour - -	360	pounds
Water for do. - - -	3540	
Weight of the wing or waftage	1000	
Leaving only - - -	1900	for the weight of the engine.
	6800	

proper movement when the machine was at anchor to the earth from some point under the prow. It ought likewise to be observed, that, owing to the internal pressure there will be exerted a power of about 460 pounds per lineal yard, endeavouring to tear the cloth asunder. This power is easily provided against by a wide netting, although most cloths would sustain it without such additional strength. Another circumstance to be adverted to, is the advantage that would arise from regulating the shape of the prow in the best manner for obviating the resistance of the air. Theory unfortunately is of little use upon this occasion, for Nature, always true to itself, makes the prow of the *bird* concave, and that of the *fish* convex to their axes; whereas theory would appoint both alike. In the absence of all good theory, I shall, as proposed in one of my former papers, give the form of the woodcock from actual measurement, that bird having frequently to cross at least 500 miles of sea at one flight, and Nature seeming to have united every contrivance to blend strength with lightness in its structure;—hence it is more than probable, that as the resistance of the air was the obstacle which all this contrivance was calculated to overcome, the external form is nearly the best possible, being that which would more than all the rest tend to the ease of the performance.

Fig. 1, Plate IV. shows a section of this bird through its axis. Fig. 2, is a tranverse section with the apparatus suspended. The *ordinates* are given in hundredth parts of an inch at the distance of one inch from each other. The weight of the individual bird corresponding with these measures was 12½ ounces, and the velocity of its leisure flight in calm air will have been about 28 miles per hour. It may possibly require a different form of prow for obviating resistance best at different velocities, and what may be the best form for a small object may not be so, even at the same velocity, for a large one; but notwithstanding this, I know no better guide, and shall therefore recommend the experiment to be made in the form of the prow delineated, as far as it can be made to agree with the flattened structure necessary in this instance for the trial of the inclined plane. That the practicability of constructing this balloon may be better judged of, I here subjoin the following estimates respecting it.

The quantity of cloth and the general appearance seem enormous; yet it must be recollected that it is only an inflated bag, and that the 5750 square yards it consists of would all pack up in a cubical bore 3½ yards each way, allowing 1-5th of an inch for the thickness of each fold, which must be amply sufficient. The cloth I made use of in my experiments generally weighed half a pound per square yard; but probably, including the netting, the estimate for the present purpose should be taken at a pound per yard; hence the weight would be 5750 pounds. Let the car, chimney, &c. be taken at 2000, and there would be the power of 3050 left unoccupied, in addition to the power of 6,800 pounds for effecting its progressive motion: however, it would be necessary for one hour's travelling to carry about 2000 pounds weight of fuel, which would only leave power to convey a crew of seven men: hence it would be more suitable to carry about 20 men, and to reduce the ascending velocity to 15 miles per hour, allowing the descent to remain at the full speed, which would reduce the general speed to 17½ miles per hour.

This statement plainly shows that the Montgolfier balloon I have described is the least that would be efficient, which I trust will shield me from the imputation of holding extravagant opinions upon this subject in proposing it. Indeed the unwieldy bulk of these bodies is unwillingly thrust upon me by the result of calculations grounded upon the facts of the case.

The danger attending the hydrogen gas balloon, where any first mover is used that acts by fire, is a great obstacle to their introduction; otherwise a balloon of this kind equal in power to the one described, would not exceed 70 yards in length, and would meet with rather less than half the resistance, and of course not consume more than half the power to drive it at the proposed velocity—there are other inconveniencies attending these balloons, and their cost in filling is not one of the least. The great resistance upon the prow must be balanced either by a firm wanded texture, or by internal condensation; the former is heavy and incapable of being folded up, and the latter wasteful, as it is impracticable to have it air-tight enough not to allow a vast escapage. This would oblige a double structure, one of thin oiled silk containing the gas, and one of a coarser texture surrounding it, which could receive the condensation necessary from common air driven in occasionally by a pump; or perhaps, with some little contrivance an aperture at the point of the prow, receiving the full direct resistance of the external air, commensurate with the velocity of the balloon, would answer this purpose. The danger from fire might be greatly reduced by having the balloon at a considerable distance above the car, say 20 yards, and the surface for waftage might be so arranged as to permit of a safe descent even in the event of the balloon taking fire, and being obliged to be cut away.

It was my intention to have ascertained what proportion of azote with hydrogen gas would render it incombustible on its access to common air; but owing to an accident in the experiment immediately previous to my leaving home, this must be reserved to a future opportunity;—perhaps the adulteration required may be so great as to render the specific gravity of the mixture too nearly the same as atmospheric air to be of any use in this instance; but if azote in the ratio of one half, or even two to one, be ascertained to be sufficient, it will be an excellent step gained towards realizing this invention.

It is evident that if aërial navigation ever be brought home to the uses of man (and who, noting the progressive stages of society, can set a limit to the powers which the benevolent Author of his being designed him by proper gradations to become possessed of?), it can only be done upon a scale of which the balloon described is the first unit; and although in the present day this unit is abundantly large enough in a *practical* point of view to occupy all the attention that can be afforded to the subject; yet I shall nevertheless obtrude a little in *prospect*, upon the duties of our posterity respecting this art, as in a doing the capabilities that remain in store for our race will operate as an encouragement towards our availing ourselves of the first step. It has already been observed, that as balloons increase in size, the diminution of their relative resistance, when compared with their power, keeps pace with that increase. Upon this principle it is easy to show that a balloon of the form delineated, inflated with hydrogen gas, when extended to the length of 144 yards, will meet with no more resistance when compared with the weight it will sustain, including its materials, than the resistance of the bird compared with its weight. Surely we cannot wish for a better basis for swift aërial navigation, than that of a vessel capable of suspending in the air as many multiples of the weight of the bird, as its resistance contains multiples of the resistance of the bird at the same velocity. This balloon would require 11880 square yards of cloth, which would fold within a cubic chest rather exceeding four yards each way, if of single structure; but I have taken it as a double structure, which would of course require two such spaces, and the weight at two pounds per yard will be 24000 pounds. I allow 17000 pounds for the other materials, including a surface of the necessary extent for waftage; when, deducting these weights, the supporting power, which is 163,000 pounds, will be reduced to 122,000 pounds, or 50 tons. This power remains to be divided in a proper ratio between the weight of the first mover and that of the cargo or crew intended to be conveyed. Although two-thirds of the weight of most birds is devoted to the muscles of its wings, and arguing from the rapid consumption of their food, that their muscles may be more energetic than those of quadrupeds, weight for weight; yet it is improbable that this excess is in any great proportion. The famous race-horse Eclipse is said to have gone for one mile at the rate of 60 miles per hour, which is a far greater velocity than any bird whose flight I have measured; and he had the disadvantage of carrying the weight of a man upon his back. If therefore the energies of birds be taken at double those of quadrupeds in this statement, more than an ample allowance is made in their favour. This power in birds is chiefly employed in the waftage necessary for their *support*, and a smaller portion of it is applied to overcome the direct resistance of their body; but in the case of the balloon the *supporting* power is already obtained, and the whole energies of the first mover will be directed towards overcoming the resistance of the prow only, which is in no greater proportion to the weight of the whole apparatus than that of the bird to its weight.

Upon estimating the probable weight even of steam-engines upon a large scale, it appears that 160 pounds per horse power is an ample allowance with its load of coal and water for one hour *; and as the water is a considerable part of the whole, and can be recovered again for the use of the engine by permitting the steam to pass within the double coats of the balloon, and to be thus exposed to so extensive a cooling surface, it is probable that 200 pounds per horse power will be more than sufficient for working twelve hours without any further supply of water or fuel—with a velocity of 20 miles per hour in calm air, this length of time would imply a stage of 240 miles. The steam-horse power raising 550 pounds one foot high per second, is considered by engineers as exceeding the average power of the largest dray-horses one-fourth. Upon weighing one of these animals of about the middle size, I found it to be fourteen hundred weight or 1568 pounds; and I am informed some of the largest of these horses have weighed a ton.—But if we take the smaller weight as approaching the average, we must increase it one-fourth part to make an animal equal in power to the steam-horse: hence the weight of the steam-horse will be as 200 pounds to 1950 pounds when compared to that of the living animal. And to follow the argument I have been using, if the

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* *Estimate of the Weight of an Expansive Steam-engine of 100 Horses Power.*

	lbs.
Weight of a cylinder 2 ft. 4 inch. diameter by 4 ft. 7 inch. in length one inch thick - - - - - - - - - -	1210
400 feet surface of tubulated boiler at ten pounds per foot -	4000
Connecting parts, piston, &c. say - - - - - -	2000
Water for an hour at 30 pounds per horse's power - -	3000
Coal for ditto at five pounds, per ditto, being more than sufficient in Mr. Woolf's engines - - - -	500
Weight of fuel constantly occupying the fire-place, say - -	1500
Weight of water occupying the boiler - - -	3000
	15210

The pressure is estimated at 20 pounds per square inch, and the stroke 4 ft. 7 inch. each second.

weight of the steam-horse be deducted, there will remain 1750 pounds, which will, according to these estimates, be the weight of inert cargo in terms of horse powers which would be conveyed by this means. This is not a correct mode of estimating the matter in question; but I wish to show, by entering into this comparison of animal and engine power, how very much the latter exceeds the former in energy, weight for weight; and that even if the estimates should be so false as to be five-fold wrong, yet there would be sufficient power for the purpose. I shall deduct three times the weight of the engine estimated in the note from the 50 tons of power unoccupied in the balloon, when there will remain rather more than 34 tons of power for any purpose required. This would convey 500 men during one hour, 410 men during 12 hours, 290 men for 24 hours, and 50 men for 48 hours, without fresh supplies of fuel or water. The extent of the voyage in calm air would in the latter case be 960 miles.

I do not offer this statement with any expectation of its being realized in our age; but I do affirm that balloon navigation does hold out the capabilities I have so daringly ventured to investigate: and I trust that others will join Mr. Edgeworth and myself in promoting experiments upon a subject that promises eventually such advantage to mankind; the progress of civilization being evidently commensurate with the facility of communication. The expense of constructing the Montgolfier balloon described in my former paper, if taken at three shillings per yard including the machinery, would be 870 pounds, and probably 1000 or 1500 pounds would defray the expenses of the experiment. The double cloth of the hydrogen gas balloon at six shillings would cost 3600 pounds, and the engines, &c. say from 6 to 7000 pounds; so that this immense fabric, when compared to the large ships it seems to outvie, is not a tenth part of their cost. Their expense, however, when compared with their freight, will be about 300*l.* per ton. The formation of hydrogen gas by the usual process is slow and expensive; but by keeping fresh supplies of iron borings red hot within an extensive cavity properly constructed, and passing steam through the hydrogen gas, may be supplied at a very cheap rate, and with considerable rapidity.

The vast strides which science of every kind has made within the last twenty years renders every advancing step more easy; and it appears to me that England may soon have the honour of perfecting the construction of balloons, although the invention of them was not altogether her own. The heavy expense, however, of such extensive experiments renders it necessary that it should become a national object; and as the means of directing balloons are within the scope of information of almost every

every engineer in this mechanical country; the only real credit that can be acquired in this pursuit, will be given to those who do actually realize the invention, and this will be due to each individual in proportion to the liberality with which he supports the undertaking.

Large individual subscriptions however are not necessary to obtain 1500*l*. and in fact subscriptions of *one* guinea are often more productive than those of a *hundred*. But whatever gentlemen may think proper to subscribe, whilst the subject is before them, I would request them to give notice of immediately, as by that means, and *that alone*, can any further step be taken before the subject will be forgotten.

I remain, sir,
Your obedient servant,
Brompton, April 5, 1816. GEO. CAYLEY.

P. S.—The sketch represents an end view of a hydrogen gas balloon with three tiers of wing to be worked by the steam-engine, or any other first mover; each wing to be divided into many stripes or portions, which are so constructed as to heel up and down by the alternate pressure of the air above and below them at each stroke of the engine, and thus by their oblique waft to propel the balloon.

LXV. *Description of Mr.* PHILLIPS LONDON's *Hydrometer; with Remarks on the curing of Mackerel for Exportation or Winter Use* *.

SIR,—I HEREWITH send you a half barrel of mackerel, part of twenty-five thousand mackerel cured on my plan; also Mr. Cutler's certificate, that so many were cured at Ramsgate during the last season, most of which were shipped for Russia. They were all cured by immersion in brine of British solid salt, agreeably to the method I had the honour of communicating to the Society, and which is printed in the thirty-second volume of the Society's Transactions, p. 204. The whole were extremely fine, and full as beautiful as the specimen sent, which I hope will meet the approbation of the Society.

I am, sir,
Your obedient servant,
PHILLIPS LONDON.
No. 57, Cannon-street, London,
Feb. 11, 1815.

To C. Taylor, M.D. Sec.

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* From *Transactions of the Society for the Encouragement of Arts, Manufactures and Commerce*, vol. xxxiii. for 1815.—The Society's silver medal set in gold was voted to Mr. London for this communication.

From Mr. London's statement at a meeting of the committee, it appears that the brine ought always to contain a redundancy of salt; in such case there is not the least danger of the fish putrefying or growing rancid, as the extra lumps of solid salt in the brine immediately act upon any watery or other liquors which proceed from the fish when inclosed in the cask. That the same process will also answer perfectly well for preserving beef or any animal food for sea store.

Certificate.

Mr. London cured by his new process under my immediate inspection upwards of 25,000 mackerel at Ramsgate, in Kent, during the last season.

JOHN CUTLER, Licensed Fish Curer,
at Ramsgate, Kent.

Sept. 28, 1814.

Reference to the Engraving of Mr. LONDON'S *Hydrometer, Plate* IV. *fig.* 3.

This instrument consists of a glass bottle, with a ground-glass stopper, to be filled with brine made from a solution of solid salt in water; within it are three glass bubbles, *a, b, c,* of different specific gravities, so graduated that, supposing the temperature of the air to be at sixty degrees of Fahrenheit's thermometer, and only one bubble floats on the surface, as shown in the engraving at *a,* it indicates the specific gravity of the brine to be 1,155, containing about 20 parts salt, and 80 of water, which is insufficient to cure animal matters with certainty by immersion in it.

When the second bubble, *c,* floats, it indicates the specific gravity of the brine to be 1,180, or about 24 parts salt, and 76 parts water, which may be used for the purpose of immersion.

But when the three bubbles, *a, b, c,* float, they indicate the specific gravity to be 1,106, or about 28 salt, and 72 water.

This brine will fully answer the purpose in the hottest weather in most climates, provided the rules be attended to which I had the honour to send to the Society last year, and the meat or fish always completely covered with the brine.

PHILLIPS LONDON.

LXVI. *On the State of the Manufacture of Sugar in France.* *By M. le Comte* CHAPTAL*.

THE last five-and-twenty years will form a memorable epocha in the annals of French industry. Most of the extraordinary events that have succeeded each other have concurred to favour its progress. France, deprived of her colonies, blockaded at all her frontiers, found herself reduced to rely on her own internal strength; and by raising a contribution of the knowledge of her inhabitants, and of the productions of her soil, she has been enabled to satisfy all her wants, to create arts which before had no existence, to improve those that were known, and to render herself independent of foreign countries for the greatest part of the articles of her consumption. Thus we have successively seen improved the arts of refining saltpetre, the manufacture of arms and of powder, of tanning leather, of spinning cotton, wool, and flax, of weaving generally, and the execution of several other arts to which we were strangers; such as the decomposition of sea-salt for the extraction of soda; the formation of alum and copperas; the fixing upon woven goods several colours which had been previously considered as fugitive; the substitution of the sugar of beet-root for that of the sugar cane; of woad in the place of the indigo plant, and of madder for the scarlet of cochineal. We might say, indeed, that the learned had diverted their attention from dwelling on public calamity, by fixing it on the means of consoling the people and lightening the burthen of their misfortunes.

Although these discoveries and many others are now become regular manufactures, it is to be feared that some of them will fall again into oblivion, either in consequence of the facility with which we can resort to the former sources, or from the habit and prejudice which recommends in the eyes of the consumer those commodities that have been a long time in use, or even by erroneous measures of administration; I therefore think it an important object that all th[illegible] processes be carefully described, in order that they may be transmitted to posterity. It will at least prove to them what science is capable of accomplishing for the prosperity of a nation at a critical period, and they may extract from it this consolatory truth, that France has the means within herself of satisfying almost all her wants.

I shall confine myself now to show how France has been enabled to supply the place of the sugar of the new world by the products of her own soil; and if the Institute approve this work, I shall have the honour of submitting to it successively all the new processes of manufacture that can be interesting to industry, to commerce, and the nation.

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* *Annales de Chimie et Physique.*

We recollect with terror those difficult times when the French, driven from the seas, had no communication either with their own colonies or those of other nations. France found herself all at once deprived of all the products of Asia and America, of which the greatest part had become articles of the first necessity. She called upon the industry of her own people; the government encouraged their efforts, and in a little time we obtained substitutes for some by indigenous products, and we found in the productions of our soil, substances absolutely of the same nature as those for which we had hitherto been dependent on the new world. The cottons of Spain, of Rome, and Naples, especially those of Castellamare, are employed in the place of those of America and India; madder takes the place of cochineal by the process of Messrs. Gouin; woad, as it is treated in the establishments of Messrs. Puymaurin, Rouqués, and Giobert, furnishes excellent indigo; and the numerous manufactories formed for the extraction of sugar from beet-root, show to Europe that we have shaken off the yoke of the new world.

Hardly were these establishments formed, scarcely were the still imperfect processes established, than another order of things took place: peace has again opened the communication, our old habits have resumed their empire, and in a little time probably we shall have banished to the rank of chimeras, the possibility of manufacturing sugar and indigo among ourselves. However, some persons have continued, and still continue, to extract sugar from beet-root: and it is easy to prove that this manufacture may be supported in competition with that of the colonies, which I believe I shall demonstrate in this memoir.

When France began to experience the want of sugar, we at first sought for the means of supplying it in the syrups of certain fruits, especially the grape, and this manufacture has been singularly improved. Large establishments were formed in several parts of the kingdom for the extraction of syrup, and they have been productive of two important results, equally advantageous; first, of causing the consumption of a great quantity of syrup in the place of sugar for several domestic purposes, and exclusively in the hospitals; secondly, of giving a value to our grapes which at that period had scarcely any. A little time afterwards a method was found of extracting a farinaceous and solid sugar from the grape, and this product was more similar to the cane sugar than the syrup; it was like the cane sugar in having no smell, and could be employed instead of it in every way, by using two or three times its weight to produce the same effect. This sugar is not susceptible of crystallization. Nearly at the same time, chemistry furnished the means of decolorating honey and depriving it of smell, so that it could be employed in the infusions of tea and coffee as well as the best syrups of sugar.

All these processes were become domestic operations, and very little privation was suffered from the scarcity of cane sugar; but it was reserved for chemistry to produce in our climate the actual sugar of the colonies, and this was not long in coming to pass. Already the analyses of Margraaf and the important labours of Achard had put us in the way; all now to be done was to improve the processes, and form a sufficient number of establishments to supply the demand. To effect this, the encouragement was prodigious, and in a single year we saw more than a hundred and fifty manufactories arise, some of which have proceeded with great success, and have poured into the market several millions of excellent sugar. The failure of a great number of these establishments may doubtless be traced to causes that must necessarily accrue on the introduction of any new species of industry, either to an error in the choice of situation, or to the great expense incurred in setting up the apparatus, or in short to the deficiency of proper information on the subject.

In the midst of a vast wreck of establishments, some are found which have continued to work prosperously for four years. In these we may reasonably expect to find lessons of practical knowledge and œconomical management, as well as the best methods of cultivating the beet-root, and of extracting sugar from it; and as my own establishment is of this number, I shall limit myself to the testimony of my own experience.

On the Culture and Preservation of the Beet-root.

It should be sown towards the end of March or in April, when there is no longer any fear of frost.

It is of different colours, white, red, yellow, or mottled, and sometimes the pellicle is red and the inside white. It is now known by all agriculturists, and especially by those of Germany, that the same colour is not always reproduced; as for example, in a field that has been sown with seed proceeding from the yellow beet, the produce has proved more or less white or red, and this I have had occasion to remark myself.

In Germany they prefer the white beet-root, in France the yellow; but in consequence of many comparative experiments I am of opinion that they give too much importance to the colour. I have not observed that the different colours produce any perceptible variation in the results, when the root proceeds from the same soil and the same culture.

The most proper soil for the cultivation of the beet-root is that which is both light and rich, and of a good depth. Poor, dry, and sandy soils are not at all suitable, for the beet comes up in such ground quite small and dry: the juice it affords marks eleven degrees by the areometer of Baumé, but is by no means plentiful. It has happened to me not to be able to extract from it more than 32 per cent. The juice is very much charged with sugar; but the proportion does not indemnify the manufacturer.

Neither is stiff argillaceous soil proper for it. The seed comes up badly, especially if soon after it is sown a heavy rain happen to fall, which heaps up the earth and prevents the access of air: in which case the seed rots without germinating. I lost in 1813 ten hectares (equal to twenty French acres) of beet-root by this accident. It is even seldom that in stiff soil the beet acquires much size; it is thrust above the surface only because there is no longer room for it below. Meadows newly ploughed and alluvial earths manured, and for a long time used, are very proper for the culture of this root. A good ground will furnish a hundred thousand of beet per hectare; I have even gathered as many as a hundred and twenty from a meadow newly ploughed; but the mean product is from forty to fifty thousand.

The ground intended to receive beet should be prepared by two or three very deep ploughings. Three years ago I sowed some in ground that was intended to receive corn in the autumn: I prepared it by two good ploughings and a suitable manuring; I sowed towards the end of March, and gathered in the beginning of October. I left the leaves upon the ground, sowed the corn, and covered it in the ordinary way; in this manner my harvest of beet-root was an intermediate harvest, which did not deprive the estate of a grain of corn.

Three years experience has convinced me that the crop of corn was equally good upon this ground, as upon that which had lain fallow during summer; and further, that the thinning, weeding, and gathering of the beet-root had cleared the soil of all weeds, and that these corn-fields were less infested with them than any other. It was for some time believed that ground newly manured produced beet-root less rich in sugar; and it has even been added, that when put into ground manured by sheep the root produces only saltpetre. I can safely affirm that these assertions are erroneous, and that the production of saltpetre is owing to another cause, which I shall demonstrate in the sequel.

Four different methods have been successively adopted for sowing the seed of the beet; but I prefer that by broad-cast, like corn, and covering it over afterwards by the harrow. This method, which is the most simple of any, is the most advantageous, although it requires a much greater quantity of seed than the others: it takes about three kilogrammes per acre, instead of one and a half; but this difference is hardly to be considered, since the price of the seed is reduced to a reasonable rate. Besides, the advantages of this method are considerable: 1st, by employing this quantity of seed we are assured that all the soil will be covered; 2dly, as soon as the plant is well up, it is weeded and thinned of all the useless roots, and only those retained that are vigorous, so that a good harvest is always certain, whatever weather it may have endured.

On the Care that Beet-root requires during its Vegetation.

Perhaps there is no plant that suffers more from the vicinity of others than the beet-root; it remains small and without vigour, if the ground be not carefully cleared of all the plants that spring up beside it.

The weeding should be renewed as often as the ground becomes covered with weeds: but in general two operations are sufficient. It is an expense and trouble well bestowed, for the produce of an acre well weeded is at least double what it would be if neglected.

In general the beet is gathered in the beginning of October, and the operation is terminated towards the fifteenth. The time of gathering is not a matter of indifference; but every one knows that in the course of vegetation there is formed a succession of different products which replace each other; so that the crystallizable sugar is contained in the beet-root only at a certain period of its vegetation, and that this period is the time that must be chosen to gather it.

In support of this opinion I can state a fact established by M. Daracq, whose talents and good sense are well known, and who formed about three years ago, in concert with the prefect of the department des Landes, M. le Comte D'Angos, the project of establishing sugar-works from beet-root.

From the month of July until towards the end of August he made a trial of the beet every eight days, and he constantly extracted three and a half per cent. of fine sugar: from these specimens he believed himself certain of success, and from that time bestowed all his care on the formation of his establishment, and discontinued his weekly trials; but he was greatly surprised, when towards the end of October he resumed his operations on the beet, to find it no longer possible to extract an atom of crystallized sugar from it. It appears, that when the beet has terminated its saccharine vegetation, if I may so express myself, it forms nitrate of potash, at the expense of the constituent principles of the sugar: and this formation takes place in the ground, when it is assisted by the heat, just the same as it does in the store-houses.

In March 1813 I wished to use some beet which I had stored in a cellar, and obtained from it only nitrate of potash, although it was neither decayed nor had germinated; this beet yielded less juice by one-third, than that which had been kept in the open air, or in magazines well aired.

It is not uncommon for puffs of nitrous gas to come out of the abundant skim which forms when the juice of the beet is poured into a boiler; the production of this gas discovers the commencement of a change in the beet, although in this state sugar may still be extracted from it: I have several times observed this phænomenon, and always in the circumstances above mentioned. By the progress of the alteration this nitrous gas passes to the state of nitric acid, this acid unites with the potash and forms nitrates, and then the decomposition of the crystallizable sugar is completed.

We must not be surprised then if in the whole of the south, from Bordeaux to Lyons, it is found that beet which has remained in the ground until the end of October, will afford only nitrate of potash, and not an atom of crystallizable sugar. As the beet-roots are pulled up, the leaves should be stripped off and left on the ground for manure, when there is not enough of them for the consumption of cattle.

Beet is as soon injured by frost as by heat: it is frozen at a temperature of one degree below zero of Reaumur; and begins to germinate and change at a temperature of eight or nine degrees above it.

In order to keep beet in a proper state it should be stored in a dry place, of a temperature a few degrees above zero of the thermometer. A barn or granary are very proper places for a magazine of this nature; but it is seldom that the whole quantity can be lodged in one building; for want of a covered place sufficiently spacious it must often be left in the open air, and when this is the case a dry piece of ground must be selected, which is not subject to be overflowed; on this ground a bed of stones should be laid, and on the stones a layer of straw; in the middle should be raised a stake, and bunches of straw placed round it that reach to the top; round this the beet should be heaped, till it forms a square of seven or eight feet by five or six in height. The stake is afterwards to be taken away, so that the space which it occupied may form a sort of chimney to give egress to the vapours that escape from the beet. The sides and the top must then be covered with a bed of straw, the top being made to incline, that the rain may not fall in or remain upon it; and the whole should be confined by bands that the wind may not have power to disturb the straw. Some cultivators, especially in the north, preserve their beet by heaping it in the fields, covering it with earth and enveloping the whole by a bed of heath or broom, so that water cannot penetrate.

But whatever method may be adopted in storing beet, there are some general precautions to be attended to which are indispensable in all cases. In the first place, it must not be stored up when wet; and if the weather will permit, it is very desirable that it should be left for a few days in the fields to dry. Secondly, it must not be covered up until frost is expected, and must be uncovered, and left so as long as the temperature is a few degrees above freezing, provided it does not rain. Thirdly, it should be often examined; and if it appears to become heated, or decayed, or germinates, the heap must be opened, the injured roots separated from it, and then it must be made up again.

On the Extraction of Sugar from Beet-root.

The extraction of sugar from this plant has given rise to a course of operations which I shall successively describe. During four years that it has been practised in France, many different processes have been employed, and great modifications have been adopted in each of the operations. I have examined and compared them all, but shall confine myself to the description of that which has constantly afforded the best results.

The beet in the state in which it is taken out of the fields is more or less clogged with earth, and the surface is more or less covered with radicles; before it is operated upon it must be freed from these incumbrances and the top cut off, which does not sensibly contain any sugar.

In some establishments the dirt is taken off by washing, and the top and radicles by a knife; but washing is tedious and expensive; it requires a great quantity of water, and the operation is difficult to perform in the severe cold of winter. The most œconomical mode of washing is to put from 100 to 140lbs. into a cylinder composed of thick iron wire, half the cylinder being immersed in water contained in a trough under it; the cylinder is kept constantly turning round. In a little time the beet is freed from the dirt, the cylinder is then raised above the trough, a door which it contains is opened, and the beet slides down an inclined plane, which carries it beyond the trough.

I have no washing in my establishment; but I have the top and radicles cut off, and the surface of the roots cleaned, all with a knife. This operation is executed with facility by women, and costs twelve sous or sixty centimes per thousand.

The sugar is extracted by two successive operations. 1st, The beet is reduced to a pulp by means of graters: the best of these graters consist of cylinders furnished on the surface with indented plates; these cylinders may be moved so rapidly, by means of wheels, that they will make 400 revolutions in a minute, and will tear and reduce the beet to a pulp in an instant. Two of these graters, put in motion by the same machinery, and attended by three women and two children, are sufficient to grate daily 10,000 weight of beet, by working only four hours a-day, two hours at a time; it is very rare that half an hour more is necessary.

Immediately as the operation of grating is terminated, the persons employed begin to cleanse the graters, to wash them, and then to place round them the 5000 weight of beet that are to be grated at the second operation.

In order that the pulp may be of a good quality, it must have the appearance of a soft paste, without any lumps; for the press, however powerful, can extract but a very small proportion of juice from fragments of beet that have not been torn. When it is only crushed between mill-stones, in the manner that is practised for making cider and perry, the juice obtained from the press is not more than 30 or 40 per cent., whereas when it is torn by the graters, from 65 to 75 per cent. is extracted. 2dly, As fast as the pulp is formed it is submitted to pressure, in order to extract the juice. I begin by putting it into small lever presses at first, and afterwards removing it to others more powerful, so as to extract from 65 to 75 per cent. of juice. The operation is perfect when the *marc* or dregs are so dry that on squeezing it hard with the hands it does not wet them. To diminish the expense of manual labour, I place the graters and presses on a stage, in such a manner that the juice falls of itself, through leaden canals, into the boilers, which are placed on the ground. It is necessary that the pulp should be expressed as fast as it forms, or else it blackens, and a degree of fermentation commences, which renders the extraction of the sugar more difficult. The juice marks from five to eleven degrees, and commonly from seven to eight by Baumé's areometer.

Four men are sufficient to work the presses, in operating upon ten thousand weight of beet per day.

I have before mentioned, that the juices run immediately out of the presses into a boiler, which I call a *depurator*, in relation to its use. Supposing two operations to be effected in a day, and that 5000 weight of beet-root is operated upon each time, this boiler, which is round, should be five feet and a half wide. and three feet eight inches deep; of these dimensions it will contain the whole product of one operation. As soon as the boiler is one-third or half full, the fire is lighted. By the time that the juice has ceased running from the presses it will already have acquired from forty to fifty degrees of heat, which is suffered to increase to sixty-five or sixty-six degrees, and the moment it has attained this heat the fire is smothered by covering it with wet coals. Lime, slaked with warm water, is then thrown into the boiler, in the proportion of two grammes and a half (about forty-eight grains) to a litre of juice, being careful to vary the proportion according to the consistence of the juice. The liquid mass must be well stirred, in all directions, for some minutes, and then the fire is revived, in order to raise the heat to eighty degrees; that is, to the degree nearest approaching to ebullition. The fire is then taken out of the fire-place, and as the liquor cools a coat forms on its surface, which in half an hour has acquired a degree of consistence, which at the end of three quarters of an hour is carefully taken off with the scum. As soon as it is skimmed, a cock is turned, which is fixed about a foot from the bottom of the boiler, and the liquor runs out into a square boiler; afterwards a second cock is opened, which is quite at the bottom of the boiler, in order to empty it entirely, and the liquor is made to fall upon a filter, through which it also runs into the square boiler.

[To be continued.]

LXVII. *On the Cosmogony of Moses.* By Mr. Andrew Horn.

To Mr. Tilloch.

Sir, — Having in my former communication shown that the term *day*, as used in designating the six periods of the Genesis, properly denotes *one* revolution, whether slow or rapid, of the earth upon its axis, which produced a *morning* and an *evening* successively upon every meridian on the globe, my object in the present paper is to rectify some misconceptions of your correspondents, respecting certain parts of the Cosmogony connected with those periods.

The statement of facts enumerated by Dr. Prichard, p. 287 of your last volume, is I conceive just, with the exception of the three articles where he says "the water *had* subsided before the creation of organized beings." But this perhaps is one of those verbal inaccuracies which he hints at in your last number Your correspondent F. E——s has committed the same mistake at p. 181 of the present volume, in which he remarks, that "the waters retire *previous* to the existence of animated beings, and never again cover the earth until the days of Noah." On this error he founds a plausible objection against the Cosmogony, and

endeavours

endeavours to support it by a reference to geological phænomena. The only passage in the Genesis, from which any such inference could be drawn, is the following, ver. 9, "Let the waters be gathered together to one place, and let the dry land appear." This construction seems somewhat to favour the error. The Hebrew, however, is in the future tense, and literally, "the waters *shall tend* to one place, and the dry land *shall appear*: and it was so." Moses uses the same original word, Gen. chap. viii. ver. 5, to describe the first appearance of the mountains at the deluge: "The waters decreased continually until the tenth month: in the tenth month, on the first day of the month, were the *tops* of the mountains *seen*." But the waters of the deluge 'were not abated from off the earth' for some months afterwards. Now it is no more possible that the earth could have been instantaneously laid dry, when the mountains emerged out of the primitive ocean, than when they showed themselves above the waters of the deluge. The formation therefore of the dry-land at the creation was gradual, and the subsidence of the waters in proportion to the means employed to drain them off. The manner in which this was effected does not at present concern us. It is evident, then, that the waters only *began* to subside, when the *dry-land appeared*. How long it was before they sunk to a fixed level never can be ascertained. Ages might have elapsed, even after the land animals were created, before the waters had completely subsided. Hence, to say nothing of the probable rapidity of the depositions, since the *period* between the elevation of the land to the complete subsidence of the waters is *indefinite*, I feel confident in the assertion, that it was sufficient for the formation of all the strata at lower and lower levels, from the clay-slate down to the chalk rocks, which are supposed to be among the last deposits of the primitive ocean; and, as the retreat of the waters was from the equatorial parts towards the polar regions, these calcareous rocks are accordingly found only in the higher latitudes.

It has been generally, but erroneously supposed that each operation was completed within the period in which it is first mentioned; and that the day was solely occupied by that particular object; whereas the subject is sometimes noticed, because it then began to be formed; at other times it is specified, because it then proved actually fit for the purpose for which it was designed. Thus the *other* is pronounced 'good' on the first day, because, as soon as it reached the earth the globe was put in motion by it. The expanse or atmosphere began to be formed, but was not completed on the second day; nothing but the other is declared 'good' or fit until the third day, in which the dry-land appeared and vegetables began to be produced; nor is the sun itself pronounced fit for its office until the end of the fourth day.

In order to correct certain mistakes, and obviate some objections to the Cosmogony, I shall, for the sake of brevity, enumerate, as Dr. Prichard has done, the facts in the Genesis, and, without drawing a parallel between them and the geological phænomena, shall only insert such observations as may be necessary for my purpose.

1. All the substances of which the earth is composed were once in a state of atomic division. The waters of the ocean universally covered for a long period the whole mass, and by chemical action formed what are called primitive rocks. But it is impossible from any present process of crystallization, to judge of the rapidity with which the original atoms were crystallized, and strata formed by their aggregation.

2. No organic beings existed in the primitive ocean till after the mountains were elevated.

3. The mountains emerged out of the ocean at the commencement of the third period. The waters then *began* to subside, but still continued to form strata, and in proportion as they left the land dry, vegetables were produced.

The production of zoöphytes and testacea is justly referred by Dr. Prichard to this epoch; because they are destitute of *locomotive* powers, which Moses positively assigns to *all* the productions of the fifth period. The objections which F. E——s has raised to this statement, when his collateral matter is removed, amounts to this—" Moses, it is true, expressly says that *all* the aquatic *locomotive* animals were produced in the fifth period, but he does not *specifically* determine in what period zoöphytes and testacea were produced; therefore the parallel fails between the facts detailed in the Genesis and those inferred from geological phænomena." But surely this is such a test as no author can endure; at most, it can in justice only be applied to those professedly systematic. Has Moses, in relating the periods of creation and order of things in the formation of this earth and its inhabitants, indeed given a false representation of nature, because he has omitted to mention one or two particular species of animated beings? Does he not at the conclusion of the narrative say, " Thus were the heavens and the earth finished, and all the host of them." This mode of expression is equivalent to that in the fourth commandment, " In six days the Lord made heaven and earth, the sea and *all* that is in them." Now it is highly unreasonable, to say the best of it, when an author gives the limits of his premises, to deny that he means to include all the intermediate terms. What should we think of the person that could question, whether another had travelled every inch of the road from London to Edinburgh, through York and Newcastle, merely because he had not named every other intervenient stage? Must the Cosmogony then be exploded, because the author has not marked every link in the chain of organization?—because he has neither pointed out the precise limit between animal and vegetative life, nor distinguished the formation of aquatic plants from those belonging to the land, nor named the species which conjoins the locomotive animals of the *fifth* with those of the *sixth* day? Should we, it seems according to the reasoning of your correspondent F. E——s, assign the *bat* species to the *fifth* day, "we place an order of locomotive beings where Moses has not placed them." But though "it is absolutely certain that Moses assigns them" to neither of these periods, he, as certainly, never designed to exclude from the Cosmogony either the bat species, which connects the locomotive animals of the *fifth* with those of the *sixth* day, or zoöphytes and testacea, which link the organized productions of the *fifth* with those of the *third* day. Having described the order in which the grand classes of organized beings were produced, he leaves us to systematize, and make our own physiological distinctions. So imperceptible is the gradation of species among organized beings, that it is only within these few years naturalists have thought proper to transfer corals, &c. from the rank of vegetables, among which they were classified as *fungitæ*, and arrange them as animals under the technical name of *zoöphytes*. Even *testacea* do not lose all analogy to vegetables, though they rank higher in the scale of existence than zoöphytes, and, agreeably to the tenor of the narrative, were probably created somewhat later. Testacea approach very near to vegetative life; their stationary disposition, and their receiving nutriment by suction, most likely from the same substances with aquatic plants, give them no small resemblance to vegetables. In short, they seem a kind of animated fungi, compared with the locomotive animals that subsist around them.

Whatever coincidence may be discovered between the above reasoning and that of Dr. Prichard, in his answer to F. E——s, is entirely accidental; for the whole paragraph was written before I received your last number. But I ought rather to have said that our agreement *necessarily* arises from an impartial view of the question.

4. The waters continuing to decrease, the vegetable kingdom became more and more extended, till the conclusion of the fourth day, when the sun by its regular operation produced a corresponding regularity in the motions of the earth, which was now

rendered fit for the reception of beings of a more perfect character than vegetables, or those motionless concretions of animated matter, zoöphytes and testacea.

5. In the fifth period aquatic locomotive animals were produced, the waters being now sufficiently purified for their reception. Dr. Prichard has well observed, that *locomotion* is the precise meaning of the Hebrew word used to distinguish this class of organized beings. It is not a little remarkable that to this period Moses assigns the production of birds. Though they are by internal organization constituted for living in air, their locomotive powers certainly have a nearer relation to those of fishes than to the powers of locomotion in land animals. Another reason, besides the perishable nature of their bones, may be assigned for their remains being so rarely found; viz. they are better provided with the means of escape from sudden inundations than land animals, or even fishes, in certain situations.

Moses notices a class of aquatic animals, in the productions of this period, which our translators have rendered 'great whales;' but literally the phrase ought to be '*great monsters;*' which will include not only existing crocodiles, hippopotami, &c. but also those large marine animals found in different parts of this country as well as upon the continent, embedded in the lyas or argillaceous limestone, and, until very lately, erroneously supposed to be crocodiles.

6. The various species of land reptiles and quadrupeds were produced early on the sixth day.

7. Every thing being thus prepared for their reception, the rational species is lastly formed; and the whole œconomy of nature being pronounced '*very good,*' man is invested by the Creator with dominion over all the inferior creatures.

It is with reluctance that I remark upon what Dr. Prichard has advanced, p. 259 of your last number. In defending Moses as an inspired writer, he says that the Egyptians were possessed of "the most authentic documents that existed concerning the history of the world." An opinion which he assumes on the authority of Michaelis, who embraces the hypothesis of Marsham in his Rule of Times,—"that Moses framed his code of laws by combining the ancient usages of the nomadic Hebrews with the institutions of the agricultural Egyptians." Now, however well calculated the hypothesis may be for displaying the learning of the German professor, his erudition is of no weight, when put in the scale against the plain and positive language of Scripture; Lev. chap. xviii. ver. 3-4. "After the doings of the land of Egypt wherein ye dwelt, shall ye not do: neither shall ye walk in their ordinances. Ye shall do my judgements, and keep mine ordinances, to walk therein: I am Jehovah your God."

Whatever use Moses might make of the Egyptian documents in composing his Cosmogony, he certainly had a purer source from whence to draw his information. The Hebrews and their ancestors were worshippers of the true God; therefore the history of the origin of the world is more likely to have been preserved pure by them than among the idolatrous Egyptians. Superstition never was favourable to truth. Traditional facts are also more safe with a nomadic than a civic people. They have fewer subjects to burthen the memory, and less temptation to corrupt them.

The perseverance of Dr. Prichard in his original intention of proving the Mosaic Cosmogony "capable of a rational and philosophical interpretation" is laudable; and the near approaches he has made to this, particularly in p. 262, 263 of your last number, are important results from the repeated attacks of his opponent. But having assumed an untenable position respecting the term *day*, he is forced, p. 260, to advance some unwarrantable opinions in order to support it. The supposition that the Hebrews were anthropomorphites must rest upon evidence from their writings. Now if we are to determine the national belief from their religious institutes and theology of their other scriptures, they were, as a nation, the furthest of all others from anthropomorphism. All other nations were idolaters, and, in general, conceived the most perfect image of the Deity must be the likeness of man. The Hebrew writers are unequalled in speaking of the spirituality of God; and where they speak of the *eyes*, *arms*, &c. of Jehovah, these expressions, in the connexion in which they stand, no more favour anthropomorphism than the term *father* itself, as applied to the Author of life. With respect to the expression "God *rested*;" it would be more agreeable to the spirit of the original, to render it *ceased*;—"God *ceased* from all his work, which he created, לעשות *to operate*," as ver. 3, chap. ii. of the Genesis, ought to be rendered.

I am, sir,
Your very obedient servant,
Wycombe, May 8, 1816. ANDREW HORN.

LXVIII. *Description of an electrical Instrument called "The Thunder-storm Alarum."* By B. M. FORSTER, *Esq.*

To Mr. Tilloch.

SIR,—I HEREWITH hand you a description and figure of an electrical instrument, which may I think with propriety be called

The Thunder-storm Alarum; as it serves to show the effect which is produced by the passage of a charged cloud over an *atmospherical electrometer.*

This instrument consists of a mahogany box about 6½ inches in height, and about 3 inches in width, also 3 inches in depth. The front (when not in use) is closed with a sliding piece of mahogany, like a sliding box lid. A glass tube A (Plate III. fig. 2,) is fixed at the top of the box, projecting some way into it; and through this a brass wire B passes, on the upper part of which is a brass ball C; and at the lower, a piece of brass D is screwed. A fine piece of flexible wire E (such as is wound round cat-gut musical strings) is inserted into the ball C; a small bell F, on a brass pillar C, is fixed at the bottom of the apparatus. A brass ball G for a clapper, is suspended by a silken string from a wire H. The inside as well as the outside of the glass tube should be coated with melted sealing-wax or other insulating substance.

There is no necessity for using an electrical machine, in order to make this instrument act: a piece of paper when dried by the fire is to be rubbed with India rubber (caoutchouc) which makes it highly electrical; the paper is then to be brought over the upper end of the flexible wire, in order to electrify the wire which passes through the glass tube; the clapper, which is suspended by the silken string, will then vibrate and strike the bell for some time, whilst the paper remains near the upper wire of the apparatus. The excited paper aptly represents an artificial charged thunder-cloud.

A piece of woollen stuff (such as is made use of for gowns), when rubbed in the same way as paper, I think acts stronger. The above-mentioned wire being flexible, may be bent, if occasion should require it, so as to come near an electrified conductor, if a *machine* is used, or near a charged jar; but the principal use of its being flexible is, that there is not the chance of accidents occurring, which might happen from a *stiff* very sharp projecting wire.

It is obvious that, if an atmospherical conducting rod were connected with this instrument, it might be used as a real thunder-storm alarum. The ball C is perforated horizontally, through which a wire may be passed to suspend a *pith-ball electrometer*, or for other purposes.

I am, sir, &c.

Jan. 22, 1816. B. M. Forster.

LXIX. *On*

LXIX. *On the Cosmogony of Moses; in answer to Dr.* PRICHARD. *By* F. E——s.

To Mr. Tilloch.

SIR,—IN your last number Dr. Prichard candidly acknowledges an immaterial inadvertence, attempts to reconcile his contradictory statements, and makes some show of maintaining his former positions; but finally, under pretence of generously extricating his opponent from an imaginary difficulty, retreats by the dubious light of Geddes's* critical torch, to a new *chateau en Espagne.* As he intimates that this castle will be respected by "*unprejudiced persons,*" I *of course* do not mean to "*attack*" it. On the other parts of his communication I shall presume to offer a few obvious remarks.

It is incidentally shown in my last paper, that his statements at different times do not always accord with each other. To this something in the way of reply has been said: not however perceiving how it invalidates my stricture, I shall leave it untouched, and content myself with pointing out more distinctly than before the contradictory passages. After noticing in his first letter the alleged extraordinary coincidence between the series of facts announced in *the six days creation*, and those inferred from geological phænomena, he remarks, that "if this coincidence is surprising in itself, it appears the more so when we compare the Cosmogony of the Hebrews with the notions on this subject that prevailed among other nations of antiquity." "We find," says he, "*invariably* that *all other speculations on this subject* are founded on some fanciful analogy with natural processes that are daily observed†." Could the most determined adversary urge any thing more in direct contradiction to this than what is contained in his succeeding letter? It is there either affirmed or shown that "*scarcely any thing* is contained in the antediluvian history of Moses *which may not also be found,* though more or less embellished, *in the records of other nations,* particularly in those of the Hindoos"—"that the Institutes of Menu begin with *an account of the creation* which bears *a strong resemblance to that of Moses,* though embellished, or deformed with many wild conceits‡"—and that, according to the information preserved in Suidas, the Etruscan Cosmogony bore a yet more striking analogy with that of the Hebrews§.

This notice of the Etruscan Cosmogony will, I hope, atone for what Dr. Prichard seems to consider a former neglect of it. He speaks, too, of my objections against the remarkable analogy that the Cosmogony of Menu bears to that of Moses *." I am unconscious of having given any opinion respecting this analogy, though I indicated the legitimate consequence of supposing the *Mosaic account* not an immediate revelation to Moses, but a *tradition* adopted by him. Dr Prichard does not deem it expedient directly to attack the position from which that consequence flowed, but asks " whether St. Matthew and St. Luke were in want of inspiration when they had recourse to previously existing documents in compiling their genealogies?" It might have occurred to him, that inspiration would in that case be superfluous. The knowledge of the genealogies was within the reach of uninspired persons, their materials being recorded in sacred writ, while the successive events of the creation could with *certainty be known to no human being* unless by *immediate inspiration.* The ingenious imagination of a sort of circuitous inspiration enabling its possessor to discriminate what had been already revealed, will scarcely bear a philosophic scrutiny: but were it even admissible, it would at least be necessary to prove the previous existence of the supposed original revelation, somewhat less vaguely than by conjectures insufficient to ascertain the person favoured with it, or even to determine the period in which it was made.

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* Whiston as a guide would have served equally well.

† Phil. Mag. No. 210, p. 289.

‡ Ibid. No. 214, p. 113.

§ Ibid. No. 214, p. 114.

Dr. Prichard does not combat my application of his *critical canon* to the assumed metaphorical sense of *day;* but with reference to the disputed meaning of that word asks, whether several expressions relating to God, such as "*God rested on the seventh day,*" are to be understood literally or figuratively. It is obvious that such expressions may *without incongruity of language* bear either a figurative or literal sense; and in fact the one or the other has been given them, according to the more or less just notions entertained of the divinity The case of the word *day* as employed in *the six days creation* is conspicuously different: if understood literally, the evening and morning connected with it have an assignable meaning; if figuratively, they become, at least to me, unintelligible. It therefore does not follow, though the Hebrew people may be suspected of having understood some expressions literally which we are accustomed to understand figuratively, that Dr. Prichard is at liberty to run counter to their acceptation of all Hebrew expressions, in violation of sense and congruity of language. As he expresses some surprise at my having left Philo and Josephus unnoticed, I shall endeavour to atone for the omission by remarking, that if he rest the metaphorical sense of the word *day* on their authority, he must also on the same authority admit a figurative sense of the *whole first chapter of Genesis*; for he says that Josephus and Philo "expressly affirm that the account of the six days work is metaphorical." Now, that the simple relation of the creation given by Moses, or what Dr. Prichard terms the "Exordium of the Hebrew Scriptures," would be rendered more "*capable of a rational and philosophical interpretation*," by being converted into an *allegory*, may perhaps not be quite obvious to ordinary understandings.

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* Phil. Mag. No. 216, p. 259.

I shall occupy but little more of your time. Dr. Prichard informs us that Moses did not write the "Cosmogony with the *Systema Naturæ* before him,"—that "the Hebrew language being very poor in terms of classification, a few leading objects in each class are mentioned; and we are left to understand that the analogous kinds were conjoined with those named." On this account he very properly retains forest trees, shrubs, and lichens in the third day's creation, although in strictness none of them come under the description of "*grass, seed-bearing herbs*, or *fruit-bearing trees*." On the same principle, were there no particular end to answer by the exclusion, it may be thought that zoöphytes might be permitted to remain in the fifth day's creation, being *moving creatures that have life*, although their motion does not *precisely accord* with the idea which, in opposition to the received translation, Dr. Prichard thinks the Septuagint and the original convey. Be this as it may, that the coincidences which he called upon us to admire were perfected by his placing orders of *beings* where Moses never placed them, remains incontrovertible. This liberty he seems to consider trifling: he has however gone still further. Even his own version of the 20th verse of the first chapter of Genesis does not exclude testacea from the fifth day's creation; a considerable portion of the order being indisputably endued with the power of loco-motion. It follows, that what he calls the coincidences "between the Cosmogony and the epochs of nature" were effected not simply by placing orders of animated beings where Moses had not placed them, but by *his* placing, at least, one order in direct contradiction to the express authority of Moses.

I am, sir,
Your very obedient servant,
Bath, May 9, 1816. F. E——s.

LXX. *Account of a Meteoric Stone which fell in the Environs of Langres. Communicated to M.* Virey *by M.* Pistollet, *Physician at Langres**.

Your love for the natural sciences, and the success with which you cultivate them, induce me to communicate to you a meteorological phænomenon which has just taken place in the commune of Chassigny, a village situated to the south-east of Langres, and about four leagues distant.

On the 3d of October 1815, at half past eight in the morning, the sky being clear and serene, and a gentle east wind prevailing, a rumbling noise was heard like the discharge of musketry and artillery. This noise, which seemed to come from the north-east, and from a cloud which hung over the horizon of an indeterminate form and a gray colour, had lasted a few minutes, when a man at work in a vineyard at some distance from the village, and who had his eyes fixed on this cloud, hearing a whistling like that of a cannon ball, saw an opaque body fall a few paces from him, and which emitted a dense smoke. Having run to the spot, he saw a deep hole in the ground, and around it were fragments of stone of a peculiar kind. Having picked up one of the pieces, he found it as hot as if it had been long exposed to a strong sun; he brought it into the village, and several inhabitants went out in consequence and collected pieces. Next day I visited the village; and having obtained one of the fragments, I found it resembled closely an aërolite which had been sent me from Germany. Having proceeded to the spot in person, I collected about sixty small pieces, some of which were soft and wet, and easily crumbled in the hand.

The ball of fire which generally accompanies aërolites, was not perceived in this instance. Having weighed all the pieces which were collected, the whole weight was four kilogrammes. I have no doubt that all these fragments belonged to the same stone; and I am even inclined to think that what I saw was only a piece of a larger stone which had exploded in the air. I am in possession of a piece weighing nearly one kilogramme, which is the half of a corner piece only, and which leads me to suppose that the whole stone must have weighed eight kilogrammes. Its very considerable specific gravity, as is remarked in all those stones, is not the same however in every fragment, some of which seem to present more density. Differences are also remarked in the colour of the crust which covers these various pieces: in some it is a very deep black, and in others of a chesnut-brown only; and in general the less black the colour is, the more shining and compact is the crust, and *vice versâ*. On the blackest crusts, elevations or swellings are observed, which have the appearance of being the produce af an ebullition suddenly interrupted.

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* *Annales de Chimie et de Physique*, tome i. Jan. 1816, p. 45.

I had almost forgot to say that some persons in the village of Chassigny and parts adjacent, who happened to be sitting on the ground, thought they felt the shock of an earthquake during the detonation; but the peasant who saw the stone fall experienced no such sensation. I ought to inform you also, that at the bottom of the hole made by the fall of the stone there was a piece of the lava of the country, which might induce one to believe that the aërolite was broken only in consequence of meeting with a hard body; but what appears to contradict this is, that none of the aërolite remained in this hole; that, on the contrary, all the fragments were dispersed in such minute pieces that it seemed rather as if they were the consequence of an explosion than of a fracture occasioned by the fall:—finally, several small pieces were imbedded deeply in the earth around the hole. Besides all this, the smoke perceived at the moment of the fall denotes something else than a simple fracture: yet it is astonishing that an explosion did not scatter the fragments farther; for one piece which was found a few days afterwards, could not have been sent there since the fall, and as a consequence of the explosion, but seems rather to have fallen at the same time with that which was separated into so many parts.

Analysis of the above Aërolite, by M. VAUQUELIN.

Physical Characters.

1. Colour: brown externally, pearl gray internally.
2. Contexture: grainy, and broken in every direction.
3. Solidity: very slight, crumbling with the greatest facility.
4. Aspect: shining, and as if varnished.
5. Sound: none. Although it appears to have been roasted, it has not the dryness nor the hardness of glass when it is broken: it seems on the contrary to be soft under the pestle, which soon pounds it.
6. It has no action on the magnetic needle, and yet the crust with which it is covered has a slight effect: this announces that it contains iron in the state of oxide.
7. It forms a jelly with the acids. Hence it must be concluded that the silex is therein combined with some principle.

Analysis.

Without entering into large details on the means employed to analyse this aërolite, I am nevertheless [illegible] that it will not be useless rapidly to explain the course which I pursued.

I subjected ten grammes of the aërolite reduced into fine powder to the action of the sulphuric acid diluted with water. The decomposition took place without effervescence:—the only phænomena observed were a development of heat and a diminution of volume in the substance submitted to experiment.

Nevertheless the whole mass did not disappear: there remained a grayish substance, which the sulphuric acid even aided by heat cannot dissolve: it forms nearly the tenth part of the aërolite.

The sulphuric acid does not acquire any colour in this operation: in the state of dilution in which it was employed, it was almost as white as water.

There are remarkable differences between this aërolite and all that have been hitherto examined.

All the latter, on being dissolved in the sulphuric or weak muriatic acid, gave out hydrogen gas partly sulphuretted, and generally furnished a solution of a green colour deeper than that of iron: the present aërolite exhibited nothing similar. This proves that this aërolite contains no iron in the metallic state which the magnet had already announced, and that there is no nickel in it, which, as we know, makes its solutions green, and is itself acted upon by the magnet.

- The solution being effected, the liquor was decanted and washed, and the residue evaporated to dryness. At the end of the operation a transparent jelly was formed, produced by the separation of the silex, which being washed and dried, weighed three grammes and one centieme.

The liquor, which ought to contain magnesia and iron, was again evaporated to dryness, and the residue calcined for at least half an hour. The matter, which had assumed a red colour, was put into water, in order to dissolve the sulphate of magnesia: it was filtered in order to collect the red oxide of iron.

The sulphate of magnesia must be evaporated and calcined three times, in order to separate entirely the sulphate of iron.

The different quantities of oxide of iron obtained in these operations having been collected and dried, weighed three grammes and ten centiemes. This oxide was of a very lively red colour.

The sulphate of magnesia when purified and calcined weighed eight grammes seven-tenths, in which there is about three grammes two-tenths of magnesia. This salt was then tolerably white, very soluble in water, and had the bitter taste which belongs to the natural sulphate of magnesia: lastly, it appeared to be pure. Nevertheless, in order to ascertain if it did not contain nickel [illegible] other metal, I put hydro-sulphuret of potash into it, which produced but a slight change of colour, which I ascribe to some traces of manganese rather than to nickel; and yet there was so small a quantity that it would not have been perceptible in the scales.

The sulphuric acid, therefore, in acting on this aërolite, had only dissolved the magnesia, the silex, and the iron.

Examination of the Residue, which was insoluble in the Sulphuric Acid.

It was interesting to examine the nature of the portion of the meteoric stone which refused to be dissolved in the sulphuric acid: it must necessarily have been of a nature different from that which had been dissolved.

This residue when examined with the glass seemed to be composed of two different substances; one in white and brilliant particles; the other black, and giving to the mixture of both a slate-gray colour.

The acids not having any action on this residue, I treated it with twice its weight of potash at a red heat. The matter when fused and cold had a greenish-yellow colour; when diluted in water it communicated to it a beautiful yellow colour, which left no doubt as to the presence of chrome. After having washed this matter until it no longer gave colour to the water I saturated the liquor with the nitric acid, and evaporated to dryness. On treating this residue again with water, the nitrate of potash and chromic acid were dissolved, and there remained [illegible], which I set aside, in order to add it to that which we had previously obtained and to that of which we are going to speak.

The nitrate of mercury put into the above yellow liquor, in fact produced a precipitate of a fine orange red.

The portion of the residue treated by potash, and which was not dissolved in the water, was treated with the nitric acid, which dissolved the greatest quantity of it: that which it left was of a very deep gray colour: the nitric solution evaporated to dryness with care, and the residue taken up with water sharpened by nitric acid, left a white powder which was still silex.

The nitric solution examined with care, did not exhibit any iron, and not an atom of nickel.

The portion of the residue which had resisted the acid and the alkali, and which had a deep gray colour, appeared to be pure metallic chrome, and perhaps with an alloy of iron: nevertheless it was not affected by the magnet. What is certain is, that when melted with borax it communicated a fine green colour to it, similar to that of common chrome.

It results from the above experiments, [illegible] meteoric stone which fell in the environs of Langres is formed in the proportion of ten grammes, as follows:

Silex	..	3·39 gr.
Oxidated iron	..	3·10
Magnesia	..	3·20
Metallic chrome	..	·20
		9·89

Or of 100 parts:

Silex	..	33·90 gr.
Oxidated iron	..	31·00
Magnesia	..	32·00
Chrome	..	2·00
		98·90

Remarks.

We remark here: 1st, that the Langres stone contains neither sulphur nor nickel, and that the iron in it is entirely oxidated; whereas all the other aërolites contain those two substances, and the iron has always been in the metallic state, at least for the most part. 2dly, That a part of the silex contained in the stone is only in the state of mixture in the sandy form, and that another more abundant portion than the first is entirely combined with the magnesia, and probably with the iron, since it is dissolved at the same time with those two bodies in the sulphuric acid. 3dly, That there is in the present stone twice as much magnesia as in those which have been hitherto analysed: perhaps its softness was owing to this cause. 4thly, Lastly, the chrome is found in it in the metallic state, which announces that it must have resisted the oxidating action which burnt the iron. The quantity of this metal is also more considerable than usual.

It is probable, that if the sulphur has existed primitively in this stone, it must have been dissipated at the moment when the iron was fused.

The absence of nickel from this stone found at Langres is the more remarkable, as it has been constantly found in all other aërolites.

LXXI. *On the Metallic Salts.* *By* G. S.

To Mr. Tilloch.

Sir, — In my last I stated, that Mr. H. *asserted the metallic salts to be [illegible] salts with excess of oxide*; I made the *same* statement in a former [illegible], and combated it without any charge of perversion: but having since been charged by Mr. H. with a misrepresentation to favour my own views, I beg you to make room for a few quotations from his paper (vol. xlv. p. 463), which I conceive to warrant my statement.

After submitting sulphat of iron to a heat sufficient to drive off a portion of the acid, he says, "Water being added, *the super sulphat was in solution, and some oxide precipitated*." Again, he says, that the sulphat and sub-sulphat of mercury "*are* not perfect salts, but *peculiar mixtures of the super sulphat with the oxide*." And, lastly, that as the sulphat of mercury is acid when water is added, "*it must* in the dry state *be an acid salt with an excess of oxide mixed with it*."

It is time to quit this subject, which must have become tedious to your readers, without affording them any real information: my design was, in the first place, to discover truth; to ascertain whether H.'s conjectures were founded on reason; and lastly, to exculpate myself from a charge of misrepresentation.

I am, sir,
Most obediently yours,
Burton Crescent, May 15, 1816. G. S.

LXXII. *A Letter from Dr.* WILLIAM RICHARDSON *to the Countess of* GOSFORD *(occasioned by the Perusal of* CUVIER'S *"Geological Essay"), describing the Arrangement of the* STRATA *for 60 Miles on the South and as many on [illegible] North of Gosford Castle, in Armagh County in Ireland. Communicated by Mr.* JOHN FAREY, *Sen.; with some preliminary Remarks and illustrative Notes by him.*

To Mr. Tilloch.

SIR, — THE printed Letter sent herewith, having been put into my hands by a Friend, I am anxious to see the same permanently preserved in your Magazine, with a few explanatory *Notes*, which I have taken the liberty of subjoining thereto, partly for the purpose of explaining, more clearly than was perhaps necessary for Dr. R. to do, to his noble Correspondent, or even to most of his Irish readers, the situations of the places mentioned, partly for referring to former Papers inserted in your Work, on this same subject, for asking some questions, &c.

Dr. Richardson, as many of your Readers know, is the author of a highly valuable and curious paper on the Basaltic Strata, and Hummocks of the Counties of Antrim [illegible] Derry, pages 102 and 194 of your thirty-third volume; another equally valuable paper on the Whynn Dykes of the same district, page 364 of your xxxvth volume; and of others.

The very able, and with him I believe, *original* manner, in which Dr. Richardson went about and completed the investigation* of the stupendous Strata and Dykes of Basalt in Antrim, under

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* The manner or *mode of investigation* here alluded to, received some years ago from me the name of *Mineral Surveying*, a British Art (originating with Mr. *Wm. Smith*) differing most essentially from the microscopic porings over the individual stones or substances met with while examining the surface of the Earth, to which *mere Mineralogists*, or mere Naturalists, however eminent or expert, are too prone, to attend to much else; and differing most essentially also, from the wild theoretical deductions from insufficient examination, to which Wernerian, Huttonian, and other partisans of the now fashionable Schools, are addicted, and whose dogmas, respectively, they too often support, with much of injustice, acrimony, and personality.

Mineral Surveying is so much more of a Mathematical, Mechanical, Graphical or Surveying art, than a Mineralogical one, (if such be understood as nicely discriminating all mineral Species,) that the term "Mineral" was prefixed to that of "Surveying," more for the purpose of marking its distinction from Land Surveying, Maritime Surveying, House Surveying, &c. than of claiming rank for its professors, amongst those technical Mineralogists, Crystallographers, Conchologists, Chemists, &c. who laudably devote their time and the energies of their minds, to the investigations and the analysis, of the Cabinet-room and the Laboratory: *all which are arts and pursuits*, I beg to repeat, *which I hold in great estimation and respect* (although time and circumstances will not admit of my particular application to them); and I have only *similar feelings towards their Professors*, individually, wherever their own personal conduct will admit of such feelings towards them, being entertained. I have said thus much here, as the only reply *I can descend to make* to the Geognostic Journalist, who at the commencement of the year, taking example and courage from another in p. 36[illegible], &c. of your last volume, unprovokedly and [illegible] has aspersed my writings, knowledge and conduct, on and relating to the hardly acquired *Profession*, by which I honestly support my Family, and benefit my Employers, as I have the satisfaction of often being told, afterwards, and particularly so, where I have happened to go over [illegible]unds on which confident *Geognosts* have preceded me

A [illegible] competent knowledge of *Rural affairs*, and of the Commerce and Mechanic *Arts and Manufactures* any way *connected with raw mineral substances*, joined with correct knowledge and expertness in *Mapping*, and in the *Geometrical investigations* of the internal structures of solid laminated Bodies, from observations made on their surfaces and in local perforations,—these I will venture to say, are of vastly greater consequence, as preliminary qualifications, for making Mineral Surveying *useful* to Land and Mineral Owners, or in useful investigations of the structure of the crust of the Earth, than all the knowledge, now or likely to be possessed by, mere technical Mineralogists, &c. great, improved and improving, and *useful also in their proper places, as I fully admit their acquirements to be.*

And I will maintain, that such a knowledge of the external characters, structures, and chemical compositions of the most *common*, and the most commonly *useful* Minerals, as is now very easily and soon to be attained, by any one, *Rurally and Commercially or practically acquainted*, as above mentioned, is all of their technical knowledge, that a Mineral Surveyor will ever find occasion for *in the field*, or while expensively exployed *out from home:* his business *then* being, to search for and explore *every* considerable or well defined Mass, whatever, *on after examination*, may turn out to be its technical name, composition, or uses, its novelty, alleged Geognostic importance or what not, to map and describe on the spot (so[illegible], as well as Map) the positions, thicknesses, contortions, dislocations, [illegible]ns, &c. of every such Mass; noteing the local name or under the very puzzling and deceptive appearances they assume, was such, as to lay impartial Geological inquirers under the greatest obligations: and few such have in consequence been able to refrain from lamenting, that now for several years past, the attention of so *very able a Geological investigator* should have been, as far as they have known, entirely diverted from this, perhaps, his proper pursuit, in which he might have rendered them and science such important services, and done himself such extensive and lasting credit, in order, perseveringly to attempt the conversion of English Farmers, unfortunately a vain attempt, as to the almost miraculous properties and produce of *Fiorin Grass*

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or names, uses, and appearances of every kind, which experience in such examinations and details will suggest; and in no instance neglecting to collect, wrap-up, mark, describe the exact locality, bring away and preserve, *sufficient Specimens of each and every substance* seen, which shall appear different from each other, of the imbedded and extraneous Fossils, &c. &c.

When returned home, and before sitting down to digest a Report, either to an Employer or to the Public in print, the *Specimens* abovementioned will be all deliberately examined, named, described, and analysed in some cases, either by the Mineral Surveyor himself, if his knowledge and Time admit (which most probably they will not) or by some more experienced and practical Mineralogist and Chemist: in neither of which respectable and useful Professions is it now very difficult, to obtain *able assistance* in the way of descriptions and analyses, on defined pecuniary terms. And thus may every useful information and result of Mineral Surveys be furnished, to private Employers or to the Public, with advantages, no other way attainable.

I must not conclude this long Note without mentioning, that the above-described process and proceeding on a Mineral Survey and Report, refers to a case, wherein *no limits had been prescribed* or wished by the Employer, *as to time or expense*, for collecting and detailing, the most perfect and ample information, regarding a particular Estate or Mineral tract; but that *more commonly* in his practice, only particular objects of inquiry and elucidation, are previously pointed out for the Mineral Surveyor, and who then goes no further into general investigation, than will materially conduce to explain the objects and extent of ground, so referred to his examination.

So also, if a Parish or larger District be ordered to be Minerally surveyed, with no *more* expense than is unavoidable, for obtaining information as to its structure, leading features and contents, the process above will be much abridged; and still more so, in conducting a County or larger Survey: but in no instance, will the careful and industrious Mineral Surveyor omit, taking and bringing home and preserving, *numerous Specimens*, from different points of every different and tolerably defined mass or stratum, and of their imbedded Fossils, &c. which he may examine. Some years previous to commencing Mineral Surveyor as a Profession, I began, and have ever since followed up this practice, until several thousand Specimens, I think, have accumulated on my hands, all accurately marked as to locality and reference to my memorandums, written on the spots: and I mean to persevere therein, with equal or more zeal than ever; although it be too much to be feared, that my time or the expense may not be spareable, for their description and arrangement, so as to be useful to others besides myself and my Sons.

I will embrace this opportunity of requesting my Friends to notice, *my Removal from Upper Crown Street*, to Howland Street, and that my Son, the Engineer (and Draughtsman and writer on Mechanical Subjects) has removed hither with me.

in Ireland; and which, yet, *I cannot doubt*, but the Doctor has faithfully related.

These circumstances, occasioned me to be much pleased at observing, from the commencement of the Letter sent herewith, that Dr. Richardson had again resumed Geological subjects, and had attentively read, and in manuscript commented on, M. Cuvier's Essay. Although I will not say with Dr. R., that "I take much pleasure in exposing the vain follies of world-making gentry," I must confess, that on reading the Essay alluded to, I saw therein, so much of hasty and unphilosophical assumption and generalization, at variance with most or all of the facts which I have yet seen for myself, on the Earth's surface or beneath it, that I beg thus to intreat Dr. R. to give publicity through your Magazine, to his *Notes* on M. Cuvier's Essay, in which case I promise to follow him with my Notes thereon.

For obviating the difficulty to which the Doctor alludes, at the conclusion of his fifth paragraph, (p. 358) I would beg to recommend to his notice, the plan which I followed in your xliid and xliiid volumes, in giving *Notes* on the first Edition of Mr. Bakewell's Geology; as being a plan, calculated, as ought in fairness to be the case, *to promote the sale*, instead of injuring it, of another's Work, from some of whose positions we may happen to dissent: and as giving, with the least possible of useless repetition, or fear of injustice, from mistaken or too scanty quotations, ample scope for free remark and criticism.

I presume to hope likewise, now that Dr. R. has returned to my favourite pursuit, he will give an attentive reading and consideration to my paper on the Strata of Antrim, compiled from his own writings and those of other Irish observers, in your xxxixth volume, and which he will find several times referred to, in my Notes sent herewith: and that he will, ere long, furnish you, at some length, with his candid and free remarks on what I have done and suggested in that paper, as to the proper classification of the Strata considered on the great scale, on their real super-positions, and their forms, as constituting *troughs* and *ridges*; the latter of which when sudden, may present the appearance of *rearing* or almost vertical strata, on their sides; and to which original cause, all of such very highly inclined strata, which I have anywhere examined, can evidently be referred, and rarely, if ever, to *faults*, as I have mentioned in the Note on p. 348 of your xlvth volume.

I am, sir,
Your obedient humble servant,
JOHN FAREY, Sen.
Mineral Surveyor.

No. 37, [redacted] near
Fitzroy-Sq[redacted]n,

Letter to the Countess of Gosford, &c.

"WHEN your ladyship was so good as to lend me the pamphlet, I return with thanks, and to encourage me to make marginal observations, I expected to have found (what I have been long looking for) materials for a controversy with the *Neptunians*, or as they are now sometimes called, the *Wernerians:* for Mr. Cuvier is avowedly of that school, and Dr. Jameson was anxious to become a missionary, and to come over from Scotland, to dissemınate in this country the *Neptunian* heresy.

"I know he has for some years been speculating on the conversion of Ireland, being rather worsted in Scotland, by the superior energy of his antagonists, supporting theories equally weak with his own, and I announced to Dr. Jameson, through a friend and pupil of his, that so soon as he published *here*, any of his Neptunian doctrines, I should instantly encounter him.

"In truth, I take much pleasure in exposing the vain follies of *world-making* gentry, and having sharply discussed the theories of the *Huttonian Plutonists*, and also of the *Volcanists*, without being able to extort a reply; I hoped for an opportunity of showing, that the *Neptunians* were just as little entitled to credit or belief.

"When an Essay on the *Theory of the Earth*, was published by Mr. Cuvier, and edited in Edinburgh by Dr. Jameson, with mineralogical notes of his own annexed, I was sure I had got what I wanted, and that I should [illegible] *Neptunian* doctrines and positions abundantly sufficient for the encounter.

"I was disappointed; Mr. Cuvier's Essay, well stuffed with curious and popular topics*, was nothing like a *Theory of the Earth*; but merely intended to give notice of the approach of his mighty work on the *Fossil bones*, found in the calcareous quarries near Paris; and as to Dr. Jameson's preface, and what he calls *mineralogical notes*; I have commented on them in the margin, with your ladyship's permission: for the public these notes would be useless without the essay, which I am not at liberty to print.

"No general statement in either, of the *opinions* supported by the *Neptunians;* no history of the formation of the earth, and arrangement of its materials, as sustained by their school; no general positions or propositions are laid down, so as to admit being controverted.

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* "I should be sorry to say any thing disrespectful of Mr. Cuvier. I am aware of the infinite labour he has expended, and the ingenuity he has exerted upon the fossil bones, found in so many calcareous strata; but as a geologist or cosmogonist, I am not yet acquainted with him, and I hope when he shall sustain positions relative to original formation and arrangement, he will sustain them from more general a[illegible] data than merely the animal exuviæ he has studied so much.

" A numerous mass of *assertions* of geological facts, is arbitrarily produced, without any reference to the places where they are to be found; besides these arrangements and facts, so decidedly pronounced, are generally *false*—mere *inventions* to countenance the Neptunian theory, in whose support they are devised.

" No other theory sets out so plausibly as the Neptunian; for its advocates show, they were once in possession of the greater part of the world, by the marine exuviæ so generally dispersed: but when we enter into detail, and examine the arrangements of our strata, and the circumstances attending them, the incompatibility of these facts, with Neptunian opinions, becomes instantly apparent; hence no doubt, they so involve themselves in loose assertions, that they are not to be caught hold of.

" As to the diversity of opinions on the formation and arrangements of the materials of our world, Mr. Cuvier gives us nine or ten pages, commencing with the 40th, containing a list of *world-makers*, who seem to have thought that Nature had whispered her secret in their ear, and that they are able to detail her proceedings, and develope all her operations; six of these are great men; the opinions of six or seven more, he states without their names, and then he tells us, he could add twenty to the list.

" Now I venture to say, every one of these philosophers has fallen into the same error, and from the same cause; they have every one looked *inwards*, and consulted their own imaginations, how things *might* be done—while not one of them looked outwards, and took a cool view of the face of Nature, to make themselves acquainted with what had actually *been done*.

" They had in their hands a mighty subject, THE WORLD, which they *assumed* to be a vast *whole*, formed and arranged by great and general causes, acting uniformly; and of course inferred that the corresponding effects must be general and uniform.

" It never occurred to these philosophers, to examine if the world before them corresponded with these ideas; nor to inquire if the *effects* were *general*, as they had assumed the *causes to be*. Had they examined, they would have found the state of things very different—no trace of a general operation—effects *local, partial, diminutive*, and *unconnected*.

" [illegible] world is to be considered as a *whole*, it will be found compos[illegible] heterogeneous parts, an infinite number of little distinct *systems*[*] or *basins*, (as Mr. Cuvier calls them) forming a diversified patchwork, or piece of Mosaic, where every separate component stone is most accurately defined, distinct, and different from the piece in contact with it, the lines of demarcation clean and delicate.

"Such is the description of a Mosaic slab[†]; and such will be found the character of our surface, when carefully examined by an unprejudiced eye, looking for the actual state of things, and not for arguments to support *favourite theories*.

"These distinct little systems pass into each other, as much *per saltum*, as the component stones of the Mosaic; not a trace of gradation on their approach, nor any rule by which it can be conjectured, what will be the material of the next system it is to come in contact with: by material I mean, the variety of *stone*, our only *solid* substance, the *Ossatura* of the world, as it has been called by naturalists.

"When we examine this aggregate of systems forming the superficial part of the world; we shall find the contiguous ones, though strongly discriminated from each other, yet often carrying with them some common properties, of Antrim and Derry the material, when not white limestone, is always basalt—the inclination of the strata generally the same, (a slight ascent to the exterior of the area,) but the arrangement and order of the strata, differ in each of the systems.

"As for instance, in the great façade of Cave-Hill[‡] there is a junction of two systems: in the one to the northward, and at the Cave, the number of basaltic strata is five or six, differing considerably from each other, as appears by their colour and tendencies to decomposition.

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[*] "I must state the sense in which I use the word *system*. I mean a well defined *area*, in which the materials and their arrangement are uniform through its *whole* extent."

[†] Dr. R. must not here be understood, by the term "Mosaic slab," to be speaking figuratively, of a slice off the surface of *the Earth* which *Moses* first described, but literally, of a work of art, a tessellated pavement. On some future occasion of more leisure, I will take an opportunity of stating my reasons of dissent from Dr. R's position, that new "systems" often (without intervening *Faults*) succeed each other without "any rule, by which it can be conjectured, what will be the material of the next system it is to come in contact with." and in the mean time request the Doctor to revise his notes or observations, and consider attentively, what is the form and nature, of *the bottoms* of the several "systems" of which he speaks: of which local tracts, I conjecture, that he has yet duly considered only the tops or surfaces, and occasionally, parts of their end sections in cliffs or precipices?:—surely, on completely crossing a "Basin," of Cuvier or other writers, we expect to see *the same stratum* or Rock emerge or rise again at the end of our journey, as we had seen dip and disappear on entering on such basin?: in other words, are not "Mineral Basins" mostly, if not all, formed in hollows or depressed parts of some individual Rock or stratum?: in "Troughs" rather, I should say, because I never yet saw *a round one*, or near to it.—J. F.

[‡] Cave-Hill façade, is situated across the Valley from [illegible], on the N. of Belfast, see Phil. Mag. xxxix. p. 270.—J. F.

" To the southward, at the park wall, the strata are thinner, more uniform, and more numerous now, in nearly the same height of façade, 12 or 13.

" The peninsula of Portrush, is an accumulation of little systems perpetually changing their materials and the order in which they are arranged.

" Such a variety of diminutive arrangements must baffle all speculation upon *general causes* and grand operations.

" I am so far guilty of the same fault I censure in Mr. Cuvier, stating arrangements, without pointing to places where they are to be verified.

" Let us look to Spain. Mr. Townsend, a modern traveller, who seems quite free from system, in his tour through that country, always states the change of material as it occurred in his passage through natural districts, from *schistus* to *limestone*, to *basalt*, to *granite*, to *sandstone*, to *gypsum*; and so on.

" What has your ladyship to do with Spain? I should not have mentioned this diversified arrangement, had I not been able to establish it in my own country, and with hopes of having your ladyship for my witness.

" You yourself are fixed on a most interesting spot*, at the southern extremity of the great basaltic area, while the Giant's Causeway itself, and our magnificent columnar façades, form its northern boundary: close to Gosford castle we find the last† remains of this curious work of nature, equally admirable whether we look to the stupendous magnificence of the *grand whole*, or the exquisite neatness of the execution.

" Your ladyship's share‡ of the Giant's Causeway is small and imperfect, as if in an evanescent state, the original features all distinguishable, but faintly marked, and not failing from decay but from defect in original execution; it being a singular feature in this curious columnar and prismatic construction, that the neatness of the work, is perpetually varying from nearly and completely amorphous, to the exquisite finishing of the Giant's Causeway itself, scarcely to be equalled by the chisel of man.

" From this spot, on which I hope your ladyship is permanently resident, proceed in any direction, and we shall try if you will encounter such an assemblage of little *systems*, as I have stated to compose the surface of our world.

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* Gosford Castle, which is N of Markethill and SE of Armagh, Phil. Mag. xxxix. [illegible] 231.—J. F.

† Mi [illegible] hern extremity of the great Basaltic area."—J. F.

‡ Ne [illegible] ford Castle, of similar strata to those, &c.—J. F.

"Go due *south*, and at the quarry of Markethill, you leave the basaltic area, and enter a schistose district, which continues for some ten miles, when you enter the gray granite district, extending from the summit of the Mourne mountain*, by Newry, and Slieve Gullian, crossing the Forkil road, but not reaching so far west, as the Newtownhamilton road.

"Near Dundalk you enter a new district of argillaceous schistus, having crossed an interesting spot well worth examining, as a little to the north-west of Dundalk three extensive calcareous districts converge towards each other: one from Carlingford to the eastward: another from Ball's Mill to the north-west; the third from Ardee to the south-west†; whether all secondary limestone, whether they actually meet at the point they converge towards, or how near they approach, remains to be inquired into.

"The Ball's Mill and Ardee districts are separated from each other by a schistose district.

"At Dundalk‡, a new and extensive district of argillaceous schistus commences; in the hills above Dunleer, they are often crossed by veins of quartz; near Drogheda we enter a calcareous district, very important to that country.

"On the south side of Drogheda we first meet the stone called *calp*, described by the Hon. Geo. Knox, in the Transactions of the Royal Irish Academy; its strata seem to me to deviate more from rectilinear planes, and to vary more suddenly in their inclination, than any other stone I am acquainted with.

"At Cloughran church we find a calcareous district, or rib, stretching§ from the foot of the hill of Howth, by Raheny and Cloughran, into the country of Meath; the boundaries of this valuable district, I am unacquainted with, but observe its component strata more inclined to the horizon than any other limestone I have met with.

"Immediately south of Dublin‖ the calp is resumed, and in three miles we enter the granite district, stretching¶ from the sea up to the mountains. The quarry stone used for building in Dublin is *calp*, and the cut stone *gray granite*.

"As we enter Wicklow county, we find a new hard stone strongly blended with *quartz*; of this Bray Head and the Sugar-Loaf Hills are formed. I could not find whether it was stratified or not; but neither in Ireland nor Scotland did I ever find granite *stratified*; a point much contested by naturalists, as the stratification of granite is necessary to some of their theories, adverse to others.

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* Situated E of Newry, toward the coast. J. F. † Or rather SSW. J. F.

‡ In proceeding again still *south*. J. F. § To the westward. J. F.

‖ See Dr. Fitton's Map, plate vii. in Ph. M. xxxix. and [illegible]ription, p. 305. J. F. ¶ To the south-westward, according to [illegible]on. J. F.

" Proceeding due west*, we again change our material in Powerscourt Park, which now becomes *micaceous schistus*, and it is over a mighty stratified façade of this stone the Powerscourt waterfall is precipitated.

" I believe the Wicklow material is mostly schistus, and on Croghan mountain, near the termination of the Military Road, I saw a beautiful junction of schistus and granite †, distinct and *rectilinear*, the granite and schistus passing into each other as much *per saltum* as the pieces of a Mosaic.

" Though your ladyship may be tired of this long tour to the southward, I must make you acquainted with the arrangements in your own immediate neighbourhood.

" Travel *northward* ‡ by Richhill road; at the end of your own wall, you enter an extensive schistose distrist, spreading to the westward beyond the Newtownhamilton road; but on the eastward for several miles, you have the basaltic area within 200 or 300 yards of you, not indeed more than two miles broad, and bounded on the eastward by a great schistose stratum, spreading into the county of Down.

" At Kilmore § you come direct on a great calcareous district, stretching from the westward of Portadown, by Loughgall and Armagh, far to the westward ||; and to the northwest by Gorestown, Clonfecle and Benburb ¶, where the Blackwater, passing through a deep ravine, shows the limestone stratum, or accumulation of strata, to be above 200 feet thick.

" Had you gone** direct to Armagh, near the town you would have crossed a new arrangement of stone, resembling basalt in colour and in its subdivisions: Mr. Weaver tells me it is argillite strongly impregnated with quartz.

" Had your ladyship stuck to the boundary of the basaltic area, it would have brought you very near both Richhill and Portadown, leaving them to the west; thence direct to Loughneagh, and across the lake to the confines of Tyrone and Derry, thence to the west side of the summit of the ridge of mountain leading due north to the ocean, where our magnificent façades commence;

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* From the Sugar-Loaf Hills. J. F.

† Quere, is this the junction mentioned by Dr. Fitton, at bottom of p. 305? or, is it on another Mountain of the same name, 6 or 7 Miles to the N of it? J. F.

‡ From Gosford Castle. J. F.

§ [illegible] Armagh, and NW of Richhill. J. F.

|| Of Kilmore. J. F.

¶ Is [illegible] the same with Benbervin Mill?, Ph. M. xxxix. p. 353. J. F.

** From Gosford Castle, or Markethill, westward. J. F.

commence; stretching for many miles to the eastward, where your ladyship admired some of them so much.

"On the southern part of this boundary[*], westward of Lough Neagh, granite seems to preponderate, and I have found red granite in abundance in the parish of Kildress; but when we enter Derry, the area for a great way to the westward is schistus, mixed with numerous quarries of stratified sandstone, and *primitive* limestone, *blue*, *crystallized* and *unstratified*.

"Had your ladyship[†] kept by the eastern boundary of the basaltic area, it would have led you, as the area increased in width, to Magheralin[‡], where you would have encountered a new variety of limestone, quite different from all I have mentioned, and particularly that near it at Kilmore; the latter a brownish red, free from flints, while the Magheralin and Moira limestone, and all the strata northward, are pure white and full of flints.

"The country or stripe to the eastward of the basaltic and limestone line, seems mostly indurated clay[§], with quarries of stratified sandstone through it, and some gypsum quarries, as far as Carrickfergus, and all crossed by many whin dykes.

"This detail must be very tedious to your ladyship, but I wish to give you every opportunity of verifying my statement, whenever you may happen to approach, or pass through any of the separate *systems* I mention; it is upon their numbers and distinctness from each other, that I deny the trace of any *great* or *general* operation on the surface of our globe so as to disturb it; but the observations which these facts give rise to, and the inferences to be drawn from them, must be reserved for another letter.

"I remain, with great respect,
"Your ladyship's very humble servant,
"W. RICHARDSON, D.D."

LXXIII. *On the relative Heights of the Levels of the Black Sea and Caspian Sea.* *By Messrs.* MAURICE ENGELHARDT *and* FRANCIS PARROT[||].

ONE of the chief objects of the travels of Messrs. Engelhardt and Parrot to Caucasus and to the Crimea, was to determine by a barometrical survey the relative height of the Black Sea and the Caspian Sea, and to measure the height of the most remarkable points of the chain of the Caucasus. The work in which they have published the fruits of their researches appeared at Berlin in 1815.

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[*] This sentence I cannot clearly comprehend: Does it mean, that in Tyrone County, near to Lough Neagh, Granite abounds?. My Map does not show Kildress; Where is it?. J. F.

[†] On leaving Gosford Castle. J. F.

[‡] Is this Maherlin, in Down County? Ph. M. xxxix. p. 377 :and where is it situated?. J. F.

[§] Query Red Marl? Ph. M. xxxix. p. 272.– J. F.

[||] Extracted from a Voyage to the Crimea and Mount [illegible]sus.—(*Reise in die Krym und den Kaukasus*) Berlin 1815. 2 vols.—[illegible]ales de *Chimie et de Physique*, tome i. Jan. 1316, p. 55.

The relative height of the two seas was ascertained twice; viz. 1st, in going from the Black Sea to the Caspian Sea; and 2dly, in returning from the Caspian Sea to the point of departure. Our travellers attempted to make corresponding observations at the level of the two seas; but this mode of verification succeeded very imperfectly.

The distance levelled from the mouth of the Kuban in the Black Sea, to the mouth of the Terek on the shores of the Caspian Sea, pursuing the sinuosities of the post road which the two travellers traversed, is 990 wersts*. If a straight line, this distance would be merely 813 wersts, corresponding to about 9½° of difference of longitude: the two extreme points are almost under the same parallel.

All the details of this vast operation are given at full length, and seem to deserve great confidence. Cistern barometers were used; but the correction of the level was made by calculation, which is very easy, the interior diameter of the tube being known.

A thermometer fixed into the mounting of the instrument gave the temperature of the mercury: another thermometer at liberty made known the temperature of the air at the moment of the observation; the scales had been rectified according to the measurement known by astronomers under the name of *fathom of Peru;* a stand made for the purpose with a leaden wire admitted of the tubes being placed in a vertical position; an anemometer gave the direction and strength of the wind, and consequently the measurement of the degree of confidence which every partial levelling seemed to deserve. The indications of the barometer are always expressed in 100dth parts of a line, and great care was taken to compare the instruments, before, after, and during the operation: in order to avoid all errors which might arise from derangements of this kind, the observers met every two days. They remained long enough at every station to take four distinct barometrical heights, and at intervals of 15′ at least: the epochs of these observations always corresponded perfectly. Every hour of the day is not equally favourable in this kind of measurement; but it did not depend on the travellers to select the hour of twelve at noon, which is generally that which answers best. Their observations, however, were always made between six in the morning and eight in the evening.

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* 101·3 wersts make 1° of the meridian.

It may be easily supposed from all these precautions that M. Parrot must have calculated his operation with great care: thus he made use of the formula of M. Laplace and of the coefficient 18393 metres which M. Ramond had found in the Pyrenees, almost under the very same parallel with the Caucasus, having in view at the same time the capillary depression of the mercury. The only correction of which he could not keep an account was that of the diminution of gravity.

The number of stations comprehended between the mouth of the Kuban and that of the Terek is 51; they were, therefore, distant from each other about 18 wersts or 13 miles. The first measurement commenced on the 13th of July, beginning with the island of Taman*, and proceeding eastward, ascending the Kuban to Batal-Paschinsk near Constantinogorks, where the 29th station was. Our travellers set out from this point to

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[*] The isle of Taman is remarkable by its springs of asphaltus, and its *foaming volcanoes*, which have been partly described by Pallas in his Travels of 1793 and 1794. Messrs. Parrot and Engelhardt visited the small volcanoes situated between the city of Taman and the lake Sucur. On the slope of a hill they discovered two basins of 16 metres aperture and two metres and a half deep, and which were filled with a frothy mass formed of argil and water. From time to time they saw rise to the surface of each crater a bubble of air about a foot in diameter: the instant it burst a great number of similar small balls took its place. This phænomenon was repeated every 30 or 40 seconds. The temperature of the water differed little from that of the air: the water at 29·4° of the centigrade thermometer, the thermometer in the sun marked 2·99°, and in the shade 26·9°. The travellers wanted proper instruments for examining the nature of the air extricated by the crater; they merely ascertained that *it was not inflammable, and that it did not keep up combustion.* The water, which was yellowish, had a saltish taste: in the bottom were found fragments of bituminous limestone, selenite, and quartzous freestone. It seems that these small basins underwent some considerable changes at the time of the great eruption which took place in 1794 in the northern part of the bay of Taman. In 1807 near Kurgan the Cossacks heard a subterraneous noise similar to a discharge of artillery. The mountain was enveloped with a thick smoke; but speedily they saw issue *slowly* from the bowels of the yawning earth a new hill, as large as a house. Large masses of calcareous stone were thrown about here and there, but no flame was perceived. In the vicinity near Bugos, fountains of asphaltus or liquid mineral tar were found, which issue from secondary layers of freestone and schistous limestone.

These phænomena of frothy volcanoes put us in mind of that of Cumacatar, on the coast of Paria; the argillaceous and impregnated plains of Chapapote (asphaltus and petroleum) of the Island of Trinity; the foaming volcanoes of Girgenti in Sicily, celebrated by the ancients; and particularly the volcanoes of Turbace, near Carthagena in New Spain, and which according to M. Humboldt send forth azotic gas much purer than is generally obtained in the laboratories of the chemists.

make excursions in the Caucasus, which lasted from the 17th of August to the 3d of October, and during which they measured the Kasbeck, a peak as high as Mount Blanc. They resumed their labours on the 4th of October, and continued them following the Terek to the delta which it forms in throwing itself into the Caspian Sea. The highest point in this operation was only 594 metres above the Taman Island. In the vicinity of Mosdock, one of the stations, and 250 wersts from the Caspian Sea, the plain was on the level of the Black Sea.

This first measurement was made under very favourable circumstances, and gave for the difference of level 105 metres by which the Caspian Sea is lower than the Black Sea.

Struck with the singularity of this result, Messrs. Parrot and Engelhardt recommenced the operation of the 10th of October, but proceeding on this occasion from east to west and following the same stations, without turning towards the Caucasus. On the 14th of October they had already gained the mouth of the Kubar. The weather during this second measurement was much less favourable, the barometer and thermometer were inconstant, the changes more sudden, the winds more variable and of more unequal strength. Snow also fell at various times: the sky was generally stormy, cold, and rainy, circumstances which usually render the altitudes too small. Now it is remarkable that it is in fact in this respect that the second determination is erroneous, for it gives 92 metres for the elevation of the level of the Black Sea over that of the Caspian Sea.

After having thus twice traversed the great Steppe, M. Parrot had the patience to make a third journey. He set out from Taman on the 24th of November, and ought to have arrived at the mouth of the Terek on the 10th of December. The want of post horses produced so many delays, however, that he did not reach the banks of the Caspian Sea until the 20th of December at 15 minutes past 11 A.M. M. Engelhardt had finished his observation on the Black Sea the night before: thus this operation presents no corresponding observations, and may merely serve as a verification. On combining the observation of Taman of the 19th at 15 minutes past 11 with the observation made next day at the same hour in the Caspian Sea, we find 99 metres for the difference of level. Another observation made at Taman in the afternoon of the 19th of December gave 102 metres. On comparing the observation of M. Parrot of the 20th with the mean of all the observations of the 19th on the Black Sea, we shall obtain almost the same difference. There is in short no barometrical observation from that of 15 minutes past 11 made at Taman from the 11th to the 19th of December, which combined with the single observation of M. Parrot of the 20th of December indicates a sensible difference of level between the two seas: the least of these determinations will not be less than 41 metres.

After having ascertained this grand difference of level, our travellers thought they should inquire if it had always existed. Now Pallas thought he recognised by the form of the strata and the shells of the Caspian Sea scattered throughout the Steppe the ancient shores of that sea. The operations of Messrs. Parrot and Engelhardt place these shores, which have an immense development, and in which we find gulfs and bays very clearly defined, at 234 metres above the present level; it must therefore be admitted that a mass of water has been lost of about 30,000 square sea leagues in surface and 100 metres in depth. M. Parrot does not think that this was by evaporation; for, according to Gmelin, the waters of the Caspian Sea are so little salt that they do not contain one *fourth* of the muriate of soda which is found in those of the Atlantic Ocean: he rather thinks that this water must have run off by rents which have been successively opened and closed, the bottom of the sea being agitated by the volcanic action, the effects of which are still seen in the Island of Taman on the Bosphorus, and at Baka on the Caspian Sea. The enormous differences between the soundings observed since 1556 and the time of Peter the Great to the present time give some probability to this opinion *.

Let us now refer these results to the operations of the same kind which have been made at different epochs in order to compare the Red Sea with the Mediterranean, the Mediterranean with the Atlantic Ocean, and this Ocean with the South Sea.

During the French expedition in Egypt, a commission of engineers of roads and bridges was charged under the direction of M. le Pere to execute the levelling of the Isthmus of Suez: thereby they resolved the celebrated question agitated from the most remote antiquity, of the elevation of the Red Sea above the Mediterranean and the soil of Lower Egypt. It results, in fact, from the labours of the commission, that the level of the

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* On taking the mean between the results of the two measurements we shall find 93½ millimetres (near 200 feet) for the quantity by which the level of the Black Sea is more elevated than that of the Caspian Sea. Hence it results that Astracan, the adjacent plains, and a very great number of other inhabited places as well in Persia as in Russia, are much inferior to the level of the Black Sea and the Mediterranean:—the singularity of this result will justify the details into which we have entered. To conclude: Before the voyage of Messrs. Parrot and Engelhardt, it had been suspected that the two seas had always one and the same level. M. Parrot's measurement corresponds perfectly with that given by Dr. Thos. Young in his Natural Philosophy. The barometrical heights of Kamychin give for the Wolga 50° 5′ of latitude, and 51¼ millimetres below the level of St. Petersburgh.

Mediterranean Sea is lower by eight millimetres at low water and nine millimetres at the high water of the Red Sea. One part of the basin of the bitter lakes is remarkable for its being eight metres lower than the level of the Mediterranean, which consequently places it 16 metres lower than the Red Sea: other points of the soil and even of the inhabited places are lower than the levels of both seas. The water of the Red Sea, for instance, might cover the whole of the surface of the Delta, and the terrors of submersion were, as has been seen, natural enough at distant periods, when this part of Egypt was less elevated than it is at present.

For want of observations exactly correspondent for estimating the difference of elevations of two very distant stations, experimentalists have sometimes used the comparison of the barometrical mean pressure: this process is capable of much precision, as M. Ramond has demonstrated, if we stop at the mean pressures of the same hours, in order to avoid the effects of the periodical variations. In order to determine the relative altitudes of the levels of the South Sea and of the Atlantic Ocean, it ought therefore to be sufficient to compare the mean heights of the barometer over the two opposite coasts of America. M. Humboldt's Journal furnishes us with the necessary data for this purpose.

In fact, we there find in the first place that at Carthagena and Cumana in the Gulf of Mexico, the mean pressure of the barometer = 0·7620 millimetre by a temperature of 25° centigrade. At the harbour of Vera Cruz, the thermometer being at 20° the height = 0·7613 millimetre, but corrected from the dilatation of the mercury, it becomes as at Cumana 0·7620 millimetre. At the temperature of zero and at the level of the Atlantic Ocean, between the tropics, the mean height of the barometer is consequently = 0·7585 millimetre.

On the shores of the South Sea, at Callao, a port of Lima, M. de Humboldt found the barometer = 0·7606 millimetre, the thermometer 20°; and at Acapulco, still on the Pacific Ocean, the barometer = 0·7617 millimetre, the thermometer 27°. These heights brought to the temperature of zero, yield for the mean pressure of the air at the level of the South Sea 0·7578 millimetre. If the slight difference which we remark between this number and that which represents the mean pressure at the level of the Atlantic Ocean could not be attributed to the errors inevitable in such delicate observations, it will result that the South Sea must be higher than the Ocean by about seven metres. Other observations of M. Humboldt which have not yet been published, will give a difference a little greater and in the same proportion; but this celebrated, traveller who only used his barometers

rometers for a geological level, thinks that in order completely to do away the doubts which the question may still present respecting the relative height of the two oceans, we must have recourse to instruments more precise, and exposed to shorter journeys by land; while we keep an account at the same time of the unequal heights of the tides, of the different hours of the observations in the harbours on the opposite shores of America, and of the horary variations of the barometer, which, although very regular as to the hours at which they happen, are not as completely so as has been supposed as to the quantities which measure them. However this may be, the observations which we have given already prove, that if there does exist a difference of level between the Ocean and the Great Pacific, it must be very small.

The little velocity and constancy which mariners have remarked in the currents of the Straits of Gibraltar, show that in these places the Mediterranean and the Ocean have nearly the same level. It may nevertheless appear curious to compare under this point of view two very distant points, since, contrary to all idea, the levelling of the isthmus of Suez, the results of which have been given above, has proved that two seas which communicate with each other may, nevertheless, have very different levels. Now the measurement of the meridian of France presents an uninterrupted chain of triangles, which extend from Dunkirk to Barcelona; the relative elevations of the various summits may be deduced from the reciprocal observations of distances from the zenith; the absolute height of one single station will therefore serve to find the absolute height of all the rest; and this will be the case, whether we set out from the Mediterranean to approach the Ocean, or follow a contrary direction.

It is according to this method that M. Delambre has calculated the elevation of Rhodes over the Mediterranean and the Ocean, setting out in the first place from Mont Juy to Barcelona, of which Stephani had directly found the height; and secondly, by the help of a signal at Dunkirk which was only 66 metres from the level of low water: those two determinations agreeing within a fraction of a metre, we may conclude, if not that the level of the Ocean at Dunkirk is exactly the same with the level of the Mediterranean at Barcelona, at least that the inequality of height, if it exists, ought to be insensible.

LXXIV. *On the Quantity of ligneous Matter which exists in some Roots and Fruits. Read to the Philomathic Society by* M. CLEMENT*.

I AM about to communicate to the Society an observation which will not perhaps be without interest, because it adds something to our knowledge of the wonderful organization of living beings, and because it may not be without utility to some of the arts.

It has been generally supposed that the ligneous matter† of some roots and some fruits which serve for our nourishment, such as potatoes, carrots, beet-root, apples and pears, form a considerable part of their mass,—a fourth or a fifth, for instance.

The husks remaining after all the cyder has been expressed from the apples, and (as a celebrated chemist (M. Chaptal) has recently informed us) the husks of beet-root, are quite well adapted to the nourishment of animals. Nevertheless it is sufficient to regard the husks of beet-roots with the slightest attention, to see that they are merely formed of small pieces of beet-root perfectly similar to those obtained by cutting and chipping the beet-root when whole with a very sharp instrument, and without allowing a single drop of liquid to escape. It is impossible not to ascertain in an instant this perfect resemblance, which strikes the eye, and which is besides confirmed by experience.

I cut a piece of beet-root into thin slices, and the latter were divided and hacked nearly to the same degree with the husks remaining after the manufacture of sugar, and which yielded 66 per cent. of juice. Nevertheless the beet-root thus cut did not furnish any liquid on being pressed; and after being dried in the sun it was found to have lost 88 per cent. of its weight, *i. e.* precisely as much as the husks dried in the same way.

Thus it is a great and mischievous error to imagine that the *mask* of beet-roots thrown away in the fabrication of sugar is a substance almost dry: it contains as much water as the entire beet-root (as comparative desiccation proves), and as much sugar as the first juice extracts; a greater mechanical division throws the whole into the liquid state. We might therefore, in the case in which the manufacture of beet-root sugar is convenient, seek with a certainty of success for the means of employing in the production of sugar that considerable portion of the root which forms about one-third of it, and which has been supposed so erroneously to be almost entirely composed of dry substances.

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* *Annales de Chimie et de Physique*, tome i. p. 173. Feb. 1816.

† By ligneous matter, I mean that substance analogous to wood, which forms the solid network of plants or fruits.

What has just been said is applicable to all the fruits employed in making cider: what has been regarded as husks, as refuse and waste, is as nearly as possible the same substance with the cider itself.

But according to this, the ligneous part which serves for an envelope or support to the liquid part of the fruits or roots which I have mentioned, will be in an excessively small quantity, since mechanical division will be sufficient to make it disappear, by causing it to float in the liquid.

In fact, this is what I have confirmed by experiments. I took off the skin from potatoes and rasped them: I washed the pulp in a sieve in order to remove the fecula; I took the husks which I put in hot water, and I added $\frac{1}{100}$dth of sulphuric acid, in order to liquefy the starch that resulted from the boiling of the fecula which remained in the husks. I filtered, after boiling some hours; and I found on the filtering paper, of dry ligneous matter, only three-fourths of a hundredth part of the weight of the potatoes. I had besides ascertained that the fecula when first separated from the husks, did not contain an atom of ligneous matter; it had formed with the acidulated boiling water a solution perfectly limpid.

I ascertained by other experiments, that the skin of common sized potatoes in one hundred parts formed only half a part of their weight.

Thus the potatoe presents the singular phænomenon of a very hard and very compact solid body, and which contains only one part and a fourth in one hundred parts of ligneous matter, having when insulated by itself the solid appearance; whereas all the rest is formed merely of starch in powder, without any adhesion, and more than $\frac{68}{100}$dths of liquid.

Apples, pears, beet-root, carrots, and many other roots or fruits exhibit also results the more astonishing, since we do not even find in them $\frac{1}{100}$dth of ligneous matter employed in forming the membranes and vessels of those organic beings, and of which $\frac{90}{100}$dths are in the liquid state.

LXXV. *Dr.* Gilby's *Description of Mr.* Street's *Patent Blowing Machine.*

To Mr. Tilloch.

Sir,—Having lately seen one of the patent blowing machines invented by John Street, Esq. of Clifton, I have been so much struck by the great simplicity and ingenuity of the contrivance, as well as by its astonishing power, that I cannot refrain from sending you a short account of it, with a request that you will allow it, if possible, a place in your Magazine.

I am, sir, most respectfully yours,

Clifton, May 1, 1816. W. H. GILBY, M.D.

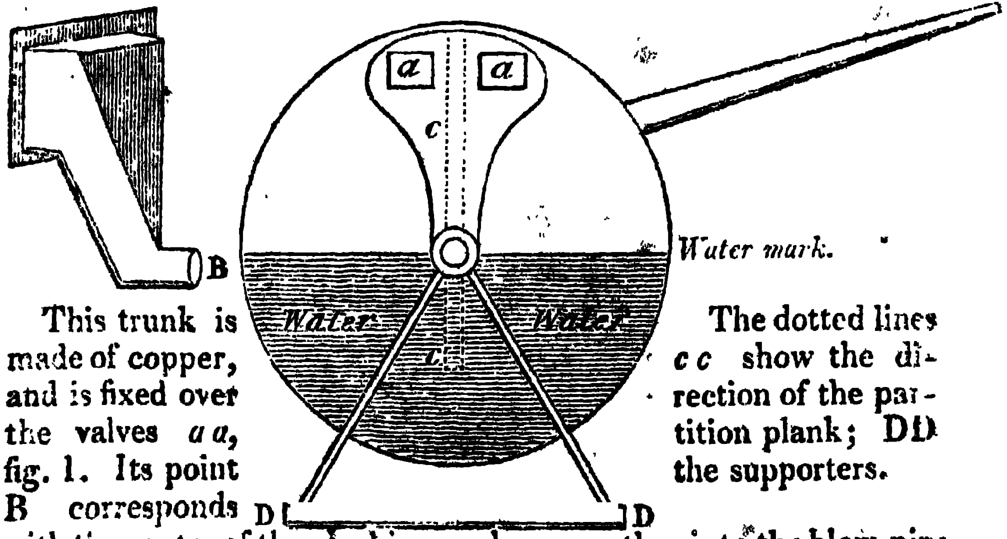
The engine itself is made of wood or metal; and to have an accurate conception of its form, we have only to imagine a drum turned sideways, and in this situation to be supported a few inches from the ground by being connected at each end with a supporter, through the means of a pivot proceeding from the centre of the parchment ends, so as to fit into a corresponding hole in the supporter. In this situation by the help of a single lever it is made to turn to and fro in the segment of a circle. From the top of the vessel placed in this lateral position there descends about two-thirds the depth of the machine a partition board, *c c*, which being firmly attached to each of the ends as well as to the top, and in this way made air-tight, serves to divide the interior into two compartments. At each end near the top there are two valves, one on each side of the partition board. At one end they open outwards, and at the other inwards, consequently the use of each set of valves is different. The lower part of the vessel is occupied by water, which is made to rise a little above the bottom of the partition board.

Having given this explanation of the construction of the vessel, we have now to enter into the description of its operation.—Having said that the machine, when in action, moves to and fro in the segment of a circle, it is quite evident, whichever way it turns, that the water will always remain at whatever part comes to be the bottom. The vessel in fact will move round the water. Such being the case, to whichever side the vessel moves, or in other words, whichever of the two compartments comes to be lowest; it is manifest, that the air included in that compartment will be powerfully compressed by the partition board descending towards the fixed body of water, and will be forced with a blast proportionate to the weight of water through the valve into a pipe. Upon the re-ascent of this compartment the same operation will take place in the other, while at the same time the air will again rush into the former by the valve opening inwards. In this way, by the rotation of the engine to and fro, the air will be alternately discharged from each compartment, and a vigorous and almost constant blast will be maintained. I say almost constant, for while the vessel is in the act of turning after the expulsion of the air from the compartment, it is obvious that there will be a momentary suspension of the blast. This inconvenience is obviated, and at the same time a great accession of blast is obtained, by using two engines, which by machinery are so contrived to work, that at the moment when the one is at the poise, or in the act of recovering itself after the expulsion of the air, the other shall be in the full energy of its operation. It is thus by the double blowing machine that a powerful and incessant blast is maintained.

The advantages of this machine in point of power, the saving of fuel and labour, and in the more complete and speedy reduction of the ore, are such, that it is found incomparably superior to the usual method of exciting an intense heat, and it is now coming into general use. Indeed its adoption is a matter of course; for it appears from the report of those who have tried it, that the manufacturer is enabled to work 40 per cent. cheaper than he can without it. The prodigious strength of the blast, and the rapidity with which the metal is reduced, are perfectly astonishing to the founder in his first experiment with it. I have seen several letters which Mr. Street has received from different manufacturers who have used it, and from the lowest statement it appears that the power of one man will melt at a cupola 5 cwt. per hour; and several have said that 8 cwt. may be fused in the same period of time. One founder computes that he can with one horse melt more metal in eight hours, than he could do before with three horses in twelve hours. In fact, nothing can be stronger and more decisive as to its advantages, than the testimonials which have been received from all quarters; and perhaps it may not be too much to say, that a discovery more beneficial to the manufacturer has not taken place since the invention of the steam-engine, the master-piece of human ingenuity.

The annexed figure represents the front end of the engine and its connexion with the supporter; and if we suppose the end to be made of glass, we should have a view of the interior, such as I have represented.

Fig. 1.



This trunk is made of copper, and is fixed over the valves *a a*, fig. 1. Its point B corresponds with the centre of the machine, and conveys the air to the blow-pipe

The dotted lines *c c* show the direction of the partition plank; DD the supporters.

LXXVI. *On the refractive and dispersive Powers of certain Liquids, and of the Vapours which they form. By Messrs.* Arago *and* Petit.

Read before the Royal Institute of France 11*th of December* 1815.

The theory of refraction, when considered under the most general point of view, is one of the most important branches of optics, not only on account of its numerous applications, but also from the consequences which may be deduced from it relative to the nature of light and to the true causes of its properties. Thus the experimentalists who have developed or supported the various systems contrived for the explanation of the phænomena of optics, are always particularly anxious to model the law of refraction according to the hypothesis which they admit.

Newton, in attributing refraction to an attraction of bodies for light, has given of this phænomenon, and of the law to which it is subjected, an explanation so clear and natural, that it has been always regarded as one of the principal arguments in favour of the system of emission. Nevertheless, if we remark that of all the general consequences deduced from the hypothesis of Newton, the only one which to this day has been verified is reduced to the law of the constant ratio of the sine of incidence and of refraction; if we observe besides that this law may be demonstrated without its being necessary to have recourse to the idea of an attraction, we shall easily perceive that before determining to adopt the hypothesis of Newton to the exclusion of all others, it is indispensable to examine to what extent the various conclusions which flow from it are confirmed by experience:—such is the object of our present researches. In order to know their object correctly, it will be necessary to retrace in few words the principal points of the theory of refraction, such as Newton has inferred from the supposition of an attraction exercised by bodies upon light.

On this hypothesis, we may easily conceive that when the molecules of which a ray is composed approach the refracting body, the attraction which it exercises on them changes both their velocity and the direction of their motion, and that this motion becomes once more uniform and rectilinear, when the molecules have penetrated into the body to the depth where the attraction ceases to be sensible. The principle of active forces applicable in this case, proves that the velocity which the light has acquired by the effect of refraction, is independent of the initial direction of the ray, and that the ratio of this velocity to that of the incident light is equal to the ratio from the sine of incidence to the sine of refraction.

The same principle of active forces gives as the measurement of the total action of the body upon light, the increment of the square of the velocity of the ray; an increment which for this reason has been designated by the name of *refractive power*. This quantity ought evidently to depend on the nature of the body; but in one and the same substance it should remain in a proportion to the density, *for it is natural to suppose that an attraction is always exercised in proportion to the mass, whatever in other respects may be the function of the distance according to which it varies.* On this supposition, the *refringent power, i. e.* the ratio of the refractive power with the density, ought no longer to depend on any thing but the chemical constitution of the body, and remain constant when the density alone changes.

This consequence of the theory of attraction has never been verified except in the gases. But if we reflect that their refractive power is extremely weak, and that consequently the increment of velocity which they impress on the light is very small, we shall be convinced, by the help of a very simple calculation, that the expression which the Newtonian theory gives for the refractive power is not the only one which, in the gases, remains in proportion to the density; but that there exists an infinity of expressions different from the latter, all of which will satisfy the same condition. It results therefore, that, so far from the gases appearing to have a refracting power independent of their density, we have no right to conclude that solid and liquid bodies possess the same property.

We are of opinion that the best method of deciding completely this question, will be to compare the refractive power of different liquids, with that of the vapours which these liquids form. In this case the change of density is very considerable, and one of the bodies at least preserves a very strong action on the light. We have therefore made choice of liquids, which at the ordinary temperatures of the air furnish the most abundant vapours. We have measured the refractive power of each of these liquids and that of the vapours which come from them: by comparing those refractive powers with the known densities of the liquids and of the vapours, it was easy to see if in each of these bodies the refracting power, *i. e.* the analytical expression

$$\begin{array}{l} \quad 2 \\ 1\text{———}1 \\ \text{————}1 \\ \quad d \end{array}$$ was independent of the density.

The result of our experiments proves rigorously the contrary. They all agree in giving for the vapours a refracting power sensibly less than that of the liquids which have formed them. Thus, in order to cite but a single example, the refracting power of the liquid carburetted sulphur, with reference to that of the air, is a little more than three, whereas that of the same substance in the state of vapour, with the same reference, does not exceed two.

If we now compare this result with the theory, we shall be obliged, by admitting the Newtonian theory of refraction, to suppose, which is at least a singular conclusion, *that the attraction of one and the same body for the light is not exerted in proportion to the density.* Unfortunately, the number of the substances on which we may operate with precision in the state of vapour, is too small to entitle us to hope to deduce from the results of our experiments, any law relative to the variation which the change of density occasions to the affinity of bodies for light. The liquids which we tried were carburetted sulphur, sulphuric ether, and muriatic ether.

In the absence of this direct method, it appeared to us that this law might be deduced from the comparison of the refracting power of the gases, and of that of the solid or liquid bodies which they form on being united. In fact, if in the combinations of gas which retain the gaseous state, the refracting power of the compound was, as has been hitherto supposed, equal to the sum of the refracting powers of its elements, it will thence result that the act of combination will not in any way modify the action of bodies on light. Hence we may conclude with probability, that the refracting power of a solid or liquid compound differs from the sum of the refracting powers of its gaseous principles, only in the ratio of the augmentation which these last undergo by the effect of condensation.

Nevertheless, as the law relative to the refracting power of the compound gases had been founded but upon a small number of experiments, it was indispensable in the first place to ascertain its exactitude: now by the measurements which we have made of the refraction of a great number of gases, we have proved that this law has not always agreed with the results of observation.

We see therefore that the refracting power of any body, far from being constant, as the Newtonian theory seemed to prove, *on the most natural hypothesis which can be formed of attraction,* undergoes on the contrary variations, either by the effect of the change of density, or by the state of combination in which the body is found. In order to determine the influence of each of those causes in particular, it is necessary to measure with exactitude the refracting powers of a great number of substances, and those of the combinations to which they give rise. Although the labour which we have undertaken on this head already embraces a considerable number of bodies, we have felt the necessity of extending our experiments still further, in order to endeavour to connect by some general law the various results which we obtained.

The facts which we have presented appeared to us to be of such importance relative to the theory of light, that we have thought it would be useful to follow the consequences of it into the various phænomena which by their nature have a more or less direct union with that of refraction.

The variously coloured rays of which white light is composed are, as we know, unequally separated from each other by their rerefraction into bodies of a different nature, and in this consists the difference of dispersive power of bodies. What is most natural to take for the measurement of the dispersive power, is the difference of the refracting powers relative to the extreme colours of the spectrum; and in the Newtonian theory this difference ought to be constant for one and the same body, as well as the refracting power of the mean rays.

Experience having shown that this last power diminished with the density, it was easy to foresee that the dispersive power would also diminish; but it was important to examine if these variations followed the same law. In order to attain this object, it was necessary to determine the dispersive power of the liquids and vapours of which we had previously measured the refracting power. The dispersive power of the liquids may be easily obtained, but it is not the same with that of the vapours. The refraction which they occasion in a prism being very weak, the dispersion, which is only a very small part of this refraction, *is scarcely sensible*. Thus, in spite of the importance of a similar determination either in the gases or in the vapours, experimentalists seem to have given up the idea of deducing it from observation. But as the object which we proposed to ourselves required a direct measurement, we were constrained to attain this object by the aid of a process of which we shall give a detailed description. We shall find besides, from the results which we shall give, that the experiments made on one and the same vapour under different circumstances, agree well enough with each other to entitle us to regard our determinations as closely approaching the truth.

On comparing the dispersive powers of the vapours thus measured, with those of the liquid from which these vapours have proceeded, we were convinced that the dispersive power was effectually diminished with the density; but what observation has taught us, in a manner not less certain, is, that the dispersive power diminishes in a greater ratio than the refracting power; or in other words, by calling i the ratio of the sine of incidence to the sine of refraction, and d the density of the body, the refracting power $\frac{i^2-1}{d}$ is not only variable for one and the same class of rays, but also the law according to which this change is effected is different for the variously coloured rays.

In the carburetted sulphur which we have already chosen for example, the ratio of the dispersive power to the refracting power is 0·14 in the liquid state, whereas it is reduced to 0·88 in the state of vapour.

Thus, while the variation of the refracting power may be also accounted for, by admitting that the attraction of one and the same body for light varies according to a law different from that of the direct ratio of the densities, we see that in order to give an account of the variation observed in the dispersive power, we must suppose besides that the action of a body on the various coloured rays follows in the changes of density a different law for each of those rays. These various suppositions diminish, without doubt, both the simplicity and the probability of the Newtonian theory; but before deciding any thing in this respect, it is necessary, we repeat, to examine with great care the changes which the refracting powers of bodies undergo, either by variations of density or by the effect of combination. It is indispensable also to join to these determinations such as are relative to the dispersive powers, to which experimentalists have not hitherto paid any attention, and which, as we have already announced, may by means of numerous precautions be deduced from direct experiments.

Although the work which we purpose to publish on this subject be very far advanced, we have thought it right to communicate beforehand the results furnished to us by our experiments on liquids and vapours.

LXXVII. *On the Use of Sulphate of Soda in the making of Glass. By the late* M. Gehlen[*].

M. Gehlen, of whose valuable services chemistry has lately been deprived by a premature death, was much occupied with the manufacture of glass, and employed the sulphate of soda as a flux. It is not the salt which is vitrified with the silex; but rather the soda, which abandons the sulphuric acid at a high temperature, on account of the chemical action which soda and silex exercise upon each other. After a great number of experiments, M. Gehlen found that the following proportions were the best:

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[*] Translated from the *Journal de Schweiger*, vol. xv. p. 89.

100 sand, 50 dry sulphate of soda, from 17 to 20 of quicklime in dry powder, and 4 of charcoal.

This composition always gives, without the addition of any other flux, a very fine glass for drinking-glasses; and we remark neither any sulphurous smell, which was sometimes so strong as to overpower the workmen, nor the extraordinary crackling which is manifested in the melting-pots when other proportions are employed.

After numerous observations M. Gehlen announces the following results:

1. The sulphate of soda may be employed in the manufacture of glass, without the addition of any other saline flux. The glass, which results is equally beautiful with that made with the usual materials, and it possesses all the properties of that made with soda.

2. The sulphate of soda alone, is but very imperfectly vitrified with the silex, even when heat is long continued. By the intermedium of lime, the vitrification succeeds better, but not in the proportion of the lime and the combustibles which must be employed.

3. The vitrification takes place, on the contrary, in a short time and in a perfect manner, by means of any substance which decomposes the sulphuric acid of the sulphate of soda, and thus destroys the powerful affinity which hindered the soda from acting on the silex. It is charcoal which best attains this object: nevertheless in the preparation of flint glass its place may be supplied by metallic lead.

4. This decomposition of the sulphate of soda may be effected during the vitrification itself, or previous. Local circumstances may determine, in the choice of one orother of those processes; but the employment of the latter presents no difficulty whatever.

LXXVIII. *Some Account of the Monuments of Thebes in Egypt*[*].

Our knowledge of the monuments of Thebes was, until lately, so limited and imperfect, that but very little could be said on the subject with any certainty. Among the numerous travellers who have visited Egypt, some have merely penetrated into the upper part, where they have but very rarely had opportunities of making extensive researches, and more rarely still leisure and talents for transmitting to us by faithful drawings what they had seen. Of all those travellers, Pocock and Norden are almost the only ones who deserve to be mentioned. But in the present day the slightest examination is sufficient to show how inadequate they are to give an accurate idea of the monuments of this country:—thus their reports furnish not the slightest idea of those wonders of antiquity with which we have been since made acquainted. It was the French expedition to Egypt which revealed to us the secrets of that country. M. Denon, by his *Voyage* and the engravings which accompanied it, was the first who gave a more precise idea of the monuments of Upper Egypt, and partly of those of Thebes*. This work fixed the attention of Europe on the country, and gave hopes of making further discoveries. The reports of M. Denon, however, only excited without satisfying our curiosity.

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[*] Translated from the Appendix to the work published by M. Heeren, of the University of Gottingen, entitled *Ideen über die Politik, den Verkehr und den Handel der Vornehmsten Voelker der Alten Welt.* 3d Edition. Gottingen. 1815.

Among so great a number of monuments, it was necessary to select and to publish only a few; and the finances of an individual, although seconded by government, prescribed certain limits to his researches.

The French government has already commenced the publication of a superb work upon Egypt ancient as well as modern, its monuments, productions, inhabitants, &c. The first number appeared in 1811. It comprehends Upper Egypt, from the southern frontier to Thebes, and is divided (like the numbers which succeeded it) into three parts: viz. Antiquities, Natural History, and Modern Statistics. The antiquities, which alone engross our attention at present, are the monuments of Philo, Elephantinus, Assuan, Esne, Edfu, Eleuthiyas, and some others less remarkable. The second number of the work is far more magnificent than the first:—here engraving seems to have made efforts to surpass herself; and 161 folios, part of them of such a size as never issued from any press before, now present us with the faithful image of that ancient Thebes where the most ancient kings of the earth resided: and if the moderns must confess that they could not now build such monuments, the architects of those distant ages, if they were to revisit the earth, could not contemplate without admiration those superb engravings of their works. The price of this work (the second number costs 1600 francs in Paris, or 80*l.* in London) precludes it from having an extensive circulation, and if it were reduced in size it would lose its splendid character.

At the moment when the great French work reached the

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* See the work now publishing by Mr. Taylor, of Hatton Garden, referred to in our last volume.—EDIT.

Gottingen library, we received that of Sir William Hamilton also, whom I had the honour to reckon among my auditors during a course of lectures which I gave on the antiquities of Egypt. The English traveller has dedicated the first part of his work to the monuments of this country, and particularly to those of Upper Egypt and of Thebes. It is true that we find here a sketch of several of the drawings of the great French work, and every person must be aware of the advantage which results from the comparison of the descriptions and criticisms, and even of the drawings of observers of two different nations, who furnish us with means of comparison so useful and important.

It is from those two sources, but chiefly the former, that I have drawn my materials. I shall endeavour, in the first place, to give a general idea of the monuments of Thebes, and to describe them afterwards in detail.

The French have measured the ground of ancient Thebes, and have given it in the *Plan General* with an exactitude which leaves almost nothing to be desired. The valley of the Nile did not present in all Upper Egypt any place more convenient for the establishment of a great capital. The chains of mountains of the two shores of the river, those of Libya on the west and the Arabic on the east, are far enough distant to leave on the two banks a spacious plain from three leagues to three and a half broad from west to east, and nearly of the same length. To the north this plain is very contracted, by the closeness of the mountains which skirt the banks; but towards the south, where the western chain leaves the river, it is open on one side. Nature had therefore put bounds to the space which Thebes occupied, but still there was space enough to make it one of the first cities of the earth. Did the ancient Thebes cover the whole of this plain? This is a question which it is difficult to decide, after all the private residences have disappeared; but as on the western side the external monuments extend to the foot of the mountains of Libya (where the Hypogæa or subterranean monuments commence), the circumstance does not appear doubtful in this part: as to the western side, where the great monuments are immediately close to the river, it cannot be determined to what extent this vast plain was covered with habitations. History teaches us to think that it was entirely so, from the extent of its population.

Thebes extended therefore over both banks of the Nile; but, so far as we know, the two parts of the city were never joined by a bridge. A nation which was not acquainted with the art of making arches, could scarcely throw a bridge over a river which is from 7 to 800 fathoms in breadth.

The sketch which we are now about to give of the monuments still existing, will be more intelligible if we distinguish them by the two sides of the river. The principal of those fine ruins bear the names of villages with which the plain is covered: on the west bank are those of Medynat-Abou and Kurnu; Luxor and Carnoc are on the opposite shore; and we must add Med-Amuth, situated at the north-east extremity of the valley, and presenting the most distant ruins. As to the size of those monuments, they resemble each other so much on both banks, that opinions are divided as to the preference which ought to be given to either.

I. *Monuments which exist in the western Part of the Plain of Thebes.*

The monuments of this part of the plain are of different kinds. They form an uninterrupted series from south to north near the Libyan chain: there remains therefore between the monuments and the river a spacious plain, which must have been covered formerly with private residences: In contemplating them we shall go from south to north.

1. *The Circus or Hippodrome* *.

The first objects which here present themselves are the remains of a grand Stadium with a small temple at the southern extremity; but the grand portico which we see on one side of this temple inclines us to think that there existed there in ancient times an edifice proportioned to those dimensions. The Circus is more than 6000 French feet long by 3000 broad. By the report of the French, the area which it presents is equal to that of the Champ de Mars at Paris taken seven times, or 624,380 square toises or fathoms: it presented therefore a space sufficient for the evolutions of an immense army. The whole had an inclosure which to this day presents a series of hillocks, through which doors and passages were made: 39 of these are still reckoned, and there must have been formerly nearly 50. The principal entrance, indicated by a larger aperture, was on the east side;

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* Sir W. Hamilton, p. 151, denies the existence of this Circus. He sees in it merely the bed of an ancient canal, also marked by the French. This space, which he values at 2000 yards in length by 40 in breadth, cannot, he says, have served for a stadium. Nevertheless the accurate researches and the measurements of the French leave no doubt as to the data of their text; and I can only account for the errors of the English author, by supposing that the inundations, which still continued, prevented him from visiting the spot completely; or, rather, Has Sir W. Hamilton taken the double inclosure of the eastern side, which leaves a space of 40 yards, for the general inclosure of the Circus? This error would be the more easily fallen into, as the inclosure of the eastern side now exists only in ruins.

the whole inclosure shows evidently that it was formerly adorned with superb architecture, which contained triumphal monuments. It is probable that this grand circus was not within the inclosure of the city, although quite close to Thebes: there was a similar Circus, but smaller, on the eastern side, almost opposite the first. If we allow that both were outside of the city, we may also determine with great probability the southern front of Thebes. It is not credible that those two establishments were destined to the gymnastic exercises solely and to chariot-racing; they were used apparently also for public meetings, and for the exercise of those armies which under the Sesostris, the Osymandyas and other conquerors, set out from thence for their conquests, and brought home there the triumphal marks of their victories.

II. On going further northward, we meet, on the edge of this sandy belt which extends along the Libyan chain, the antiquities of Medynat-Abou. I comprise under this name, proceeding from south to north;

a. A palace and a temple almost at the northern extremity of the Circus.

b. The Colossus of Memnon, with the other Colossi quite near, and the remains of a building which seems to be the *Memnonium* of Strabo.

c. The palace and tomb of Osymandyas, frequently called Memnonium by travellers.

All these monuments are almost at the foot of the Libyan chain, and 1500 toises from the hill.

We shall in the first instance take up the subject of the *palace*, the *pavilion* which communicates with it, and the *temple*. It is a matter of great historical importance to meet with these edifices, the interior arrangement of which proves in an incontestable manner that they were not temples properly so called, but buildings destined to be inhabited, and probably by kings. The pavilion is a building of two stories; it has several halls and chambers, and a great number of windows. Its position is so well adapted, that from this point we perceive not only the monuments of Medynat-Abou, but also those of the other side of the Nile and the whole plain of Thebes. Every thing there seems to indicate the ordinary residences of Kings: the ornaments of the sides in particular confirm this hypothesis. These representations differ essentially from those which are found in the temples: they exhibit in part family scenes. Unfortunately this edifice is very much damaged, and the upper story only is preserved.

It is 250 feet north-west from this pavilion that the grand palace of Medynat-Abou is to be found. Its entrance is formed by those masses of architecture, foreign to the European style it is true, but known in France by the name of *Pylones*, and which the Greeks also called *Propyleæ*. There are here also two truncated pyramids 66 feet high, which contain between them the principal gate and grand entrance. This gate leads into a spacious court surrounded by galleries, formed on one side by eight columns, on the other by pilasters, on which are affixed as caryatides, but without bearing any thing, some colossal statues of Osiris. The aspect of the whole inspires, according to the report of eye-witnesses, a sentiment of ineffable veneration. Opposite to the principal entrance rises a second *pylone* of a smaller [illegible] it leads into a second peristyle, also formed of caryatides, pilasters, and columns.

[To be continued.]

LXXIX. *On a curious Property of vulgar Fractions.* By *Mr.* J. Farey, *Sen.*

To Mr. Tilloch.

Sir, — On examining lately, some very curious and elaborate Tables of "Complete decimal Quotients," calculated by Henry Goodwyn, Esq. of Blackheath, of which he has printed a copious specimen, for private circulation among curious and practical calculators, preparatory to the printing of the whole of these useful Tables, if sufficient encouragement, either public or individual, should appear to warrant such a step: I was fortunate while so doing, to deduce from them the following general property; viz.

If all the possible vulgar fractions of different values, whose greatest denominator (when in their lowest terms) does not exceed any given number, be arranged in the order of their values, or quotients; then if both the numerator and the denominator of any fraction therein, be added to the numerator and the denominator, respectively, of the fraction next but one to it (on either side), the sums will give the fraction next to it; although, perhaps, not in its lowest terms.

For example, if 5 be the greatest denominator given; then are all the possible fractions, when arranged, $\frac{1}{5}$, $\frac{1}{4}$, $\frac{1}{3}$, $\frac{2}{5}$, $\frac{1}{2}$, $\frac{3}{5}$, $\frac{2}{3}$, $\frac{3}{4}$, and $\frac{4}{5}$; taking $\frac{1}{3}$ as the given fraction, we have $\frac{1+1}{5+3} = \frac{2}{8} = \frac{1}{4}$ the next smaller fraction then $\frac{1}{3}$; or, $\frac{1+1}{3+2} = \frac{2}{5}$, the next larger fraction to $\frac{1}{3}$. Again, if 99 be the largest denominator, then, in a part of the arranged Table, we should have $\frac{15}{52}$, $\frac{28}{97}$, $\frac{13}{45}$, $\frac{24}{83}$, $\frac{11}{38}$, &c.; and if the third of these fractions be given, we have $\frac{15+13}{52+45} = \frac{28}{97}$ the second: or $\frac{13+11}{45+38} = \frac{24}{83}$ the fourth of them: and so in all the other cases.

I am not acquainted, whether this curious property of vulgar fractions has been before pointed out?; or whether it may admit of any easy or general demonstration?; which are points on which I should be glad to learn the sentiments of some of your mathematical readers; and am

Sir,
Your obedient humble servant,
Howland-street. J. Farey.

LXXX. *Notices respecting New Books.*

Ordnance Maps of British Counties.

The circumstances which were thought to render expedient the suspension of the publication of the ordnance maps being now removed, the publication of them is resumed, and they may be obtained, as formerly, at the Drawing-Room in the Tower, or of Mr. Faden, Charing-Cross. As the suspension was only intended to be temporary, not merely the operations of the Trigonometrical Survey, but those of the mapping and engraving, have been regularly carried on during that period, under the superintendance of Colonel Mudge; so that several county maps will be ready for delivery almost immediately. The maps of Cornwall, Devonshire, Dorsetshire, Hampshire, (including the Isle of Wight,) Sussex, and that part of the county of Kent which squares-in on the Sussex side with the general work, will be published in a very few weeks: and a separate map of the Isle of Wight is now on sale. The maps of all the contiguous counties north of these are in the hands of the engravers; and that of the whole county of Kent is re-engraving, and in a state of forwardness. When the several plots and portions now planning by the surveyors are finished, at least three-fifths of England and Wales will be ready to be placed successively in the hands of the engravers; and the work will be carried on with all possible expedition consistent with accuracy. These maps are on a scale of an inch to a mile, a scale that admits of an attention to minutiæ which must of necessity be disregarded in maps of smaller size. Hence, it may not only be expected that the general outline and the prominent physical circumstances shall be correctly delineated, but that the minuter points and peculiarities which are interesting to the topographer and the antiquarian shall be permanently marked and readily traced in these maps.

LXXXI. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

May 2. Dr. Nixon communicated an account by Dr. Serres of a singular case of complete euphony cured by electricity. The subject was a young French officer, who at the battle of Dresden was in the act of giving the word of command, when a ball passed him, the effect of which on the air knocked him down, at the same time rendering him speechless, and for a day almost insensible. Two men near him were killed by the ball, which did not touch him. In the hospital at Dresden he partially recovered the use of his left side and hearing, which were impaired; but all efforts to recover his voice were in vain, and he was discharged as an invalid. His hearing was still very confused, but his smell was preternaturally acute, and the smell of coffee was altogether intolerable to him. His tongue had contracted into a small protuberance in his mouth about the compass of an inch, and his left side continued benumbed, till he was prevailed on to be electrified by Mr Timman of Brussels. He had not been electrified above seven or eight times when his hearing was improved, and his tongue began to expand. Mr. T. then passed shocks through his mouth and down to his stomach, when he hastily got up, and in a low voice returned thanks to the operator, and ran off to Amsterdam like a person deranged. He returned, however, in a few days perfectly cured in his voice, which is now better than it was before the accident; but he still feels some pain in his left leg, and occasionally in foggy weather an oppression on the chest. But after eight months and twenty-five days he recovered his voice completely.

Dr. Wollaston read a paper, containing his remarks on the glazier's diamond, and an account of his experiments with that instrument. He discovered that the diamonds for cutting glass are all in a natural state, and not cut by the lapidaries; that the natural angles of the stones are probably harder than the artificial; that the surface of all cutting diamonds is curvilinear; that the groove which they make (for it is not a scratch as commonly supposed) on the surface of the glass is a tangent to the face of the diamond; and that flint and other hard stones may be formed so as to cut glass the same as a diamond, but that their durability is shorter in proportion as they are softer.

May 9. A letter to the President by —— Chapman, Esq. M.R.I.A. was read, detailing some observations and conjectures on the geological formation of coals. The author had reason to suppose that coals were formed from peat; and to illustrate this opinion he has observed the depth of peat bogs in Ireland, and in the north of England, and compared the results with the coal-mines at Newcastle, whence he writes. The deepest peat-bogs are from 30 to 40 feet; and he finds by calculation that if this mass was compressed, it would be about equal to the strata of coal at Newcastle, although it is far from equalling those in Staffordshire. He also traced the analogy between the timber or trees found in peat-bogs and on the sea-shores of Northumberland, and the gritstone found in the Canton mine at Newcastle. This stone, specimens of which have been sent to the British Museum, has the perfect form and appearance of trees; and even its apparent fibres are such as to leave no doubt of the kind of wood which had preceded the present sand or grit. It appears that this ligniform stone fell from the roof of the pit, and had been in an erect position, the same as trees are often found in peat-bogs in the natural mode of growth. In falling, these stones leave behind them the model of their bark, which the author thinks has been converted into canal coal. He also took the specific gravity of peat, which he found to be in general 1200, and noticed the peculiar oval form into which trees are compressed in the earth. The combustion which assisted the change of peat into coal, he considers, may have been effected by means of the martial pyrites.

May 16. A letter from Mr. Mornley to Dr. Wollaston was read, describing an immense block of meteoric iron found in Brazil, about 50 leagues from Bahia. This extraordinary mass was discovered near the bed of a river, in a sterile country, where the granite rocks occasionally surmount, and are never more than twelve feet below the surface of the soil; there are few trees, and those stunted; and hedges are formed of a species of *euphorbia*, the juice of which emits a phosphorescent light, and is highly deleterious to the skins of animals. The block of meteoric iron measured about 6 feet by 4, and the author calculated that it contained 28 cubic feet, and weighed 14,000lbs. An unsuccessful attempt was made about thirty years ago to transport it to Bahia, and for this purpose forty pair of oxen were employed; but the apparatus failed, and the attempt was abandoned. It was removed only some yards, and now lies in lat. 10° 33′ south. There are thermal springs in that country, the temperature of which was from 81 to 101, the atmosphere being 77 and 88;

the water contains iron, is bitter, and clear. Common salt abounds, and is collected by the inhabitants; but it is bitter, and purgative to those unaccustomed to its use. Dr. Wollaston analysed the specimen which the author succeeded in chiseling from the block, which he found to be magnetic. In Dr. W's analysis he found it to contain about 4 per cent. of nickel; the mineral is crystallized; he dissolved it in nitric acid, added ammonia, and precipitated it with a triple prussiat. The specimen given to Dr. W. he found to be susceptible of magnetism, like all native iron ore; he therefore confirms the author's observations on the magnetism of the entire block, which does not at present lie in the direction of its poles.

May 23. A letter to the President from T. A. Knight, Esq. F.R.S. was read, containing his observations on ice found in the bottom of running water. Last February Mr. K. observed near his residence, that ice was attached to stones at the bottom of a river, while the surface was only covered with innumerable spiculæ, but not converted into solid ice. He likewise found ice below water near a mill, where the water had been precipitated over stones: from the circumstance of the water having spiculæ running on its surface, and its temperature being quite as low as the freezing point, he is inclined to think that these spiculæ have been carried to the bottom by eddies and water-falls, and that coming in contact with certain stones somewhat cooler than the water, in this manner solid ice was formed at the bottom of rivers. If, however, ice has been found at the bottom of stagnant water, he admitted that this theory would be inadequate to explain the phænomenon. Of the latter circumstance, indeed, he appeared to have no perfect knowledge.

Sir Everard Home, Bart. furnished a curious paper on the formation and use of fat in the tadpole. In his researches he was assisted by Mr. Hatchet, who analysed the eggs or ova of frogs, and discovered that they have no yolk. The tadpole of the *Rana paradoxa* is so large that its internal structure can easily be examined, and it is sold in the market of Surinam for fish. Sir E. having obtained specimens of those tadpoles examined their anatomical characters, described the fins or fringes which serve as gills, noting the different appearances in their metamorphoses, the formation of the head of a perfect frog; and finally, the extinction of the tail. The common tadpole, it appears, lives on the gelatinous matter with which it is surrounded; and to this end it is provided with a comparatively very long gut, which enables it to take up food; and that this gut is lined with fat, which in the animal's progress from the tadpole to the frog gradually disappears; in 20 days it loses those fringes which served it for gills, and acquires a head and lungs; in seven more its tail

falls

falls off, and the fat of its intestine is then entirely consumed. The length of the gut and its fat, Sir E. thinks, supply the want of yolk in the ova. Mr. Hatchet's experiments on ova presented some curious facts; he found the yolk to consist of oil and a peculiar animal matter. In some cases the animal matter was of a yellow colour indestructible by alkalies, and might be used for marking linen; the gelatinous mass is of a nature between albumen and gelatin.

SOCIETY OF ARTS.

Ventilation of Mines.—On Wednesday the 23d instant the Society of Arts voted their gold medal and a reward of 100*l.* to Mr. Ryan, for his improved system for ventilating mines. This is the highest honour and pecuniary reward which the Society can bestow upon an individual, and strongly marks the high sense they entertain of the benefits likely to result from the general introduction of this system into all the coal-mines. Prejudice, ignorance, and mistaken interest, must fall before the light of science and the calls of humanity. The original object of Mr. Ryan was the prevention of explosions in coal-mines, and the preservation of the health of miners. The safe-lamp of Sir H. Davy has effectually accomplished the first—the system of Mr. Ryan accomplishes the second; with this additional advantage to coal owners, that they may now work out the entire bed of coal, and not leave behind them about one half as pillars to support the roof: and this will yield another benefit—the roof and floor being allowed to close, the cavity will be so much diminished as not to form a reservoir for water, threatening the drowning of neighbouring mines, with the death of all the workmen, as was the case some months ago in the Heaton Colliery.

ROYAL INSTITUTE OF FRANCE.

Analysis of the Labours of the Class of Mathematical and Physical Sciences for the Year 1815. *By* M. Cuvier.

CHEMISTRY.

[Continued from p. 312.]

M. Gay Lussac has also presented to the class some papers on the cold which results from evaporation, and on evaporation in the air at different degrees of temperature and pressure, in which he expresses, by a formula, the results of his experiments. He followed up his last paper with one on hydrometers, which contains the immediate consequences of the foregoing memoirs; but these works not having, as he thinks, as yet acquired that precision and order which he has been accustomed to give to all that he publishes, he has deferred their publication.

M. Dulong of Alfort has detailed some experiments on the oxalic acid, which, although they do not form a complete series, nevertheless hold out some interesting prospects in science. On saturating this acid with barytes, strontian, or lime, salts are obtained which always represent the acid employed, even after they have been exposed to a heat superior to that of boiling water; but with oxide of lead or zinc we always lose twenty per cent. of the acid by desiccation. On burning these dried metallic oxalates afterwards, no water is obtained; but we obtain carbonic acid, gaseous oxide of carbon, and there remain the oxides of the metals employed, among which that of lead presents peculiar properties. The oxalates of copper, silver, and mercury, always give, on the contrary, water on decomposition, however much dried, at the same time with carbonic acid, and the residue is in the metallic state. A detonation is produced by the oxalate of silver, and we know besides that it detonates on being compressed, as well as the oxalates of mercury.

As to the oxalates of barytes, strontian, and lime, they give on being decomposed by heat, empyreumatic oil, water, oxide of carbon, carbonated hydrogen, and carbonic acid, and there remains a mixture of subcarbonate and charcoal.

These phænomena may be accounted for in two ways:

Either the oxalic acid must be composed solely of carbon and oxygen, in proportions intermediate between those of the carbonic acid and the oxide of carbon; but it contains water, which certain oxalates, like those of lead and zinc, give off when dried, while the rest retain it; or rather it must be composed of carbonic acid and hydrogen. This last with the oxygen of the oxide will form water, which the first oxalates allow to escape, and there will only remain the carbonic acid and the metal, a new combination in chemistry; for it has been generally understood that the metals cannot be united with the acids until they are oxidated. M. Dulong, who inclines towards this last explanation, therefore, thinks that these oxalates of lead and of zinc, when dried, are not true oxalates; and he proposes to give them, as well as to any combinations of the same kind which may be discovered, the name of *carbonides*. As to the oxalates which do not give off water in drying, they will contain the oxalic acid entire; and as after its composition it will in future be called hydrocarbonic, the salts themselves will be called *hydrocarbonates*.

M. Dulong has been led by analogy to very general conclusions, by which he brings under the same laws not only the common acids, but also the hydracids; but we shall give a more detailed report when he has sent us the full accounts which he has promised.

The chemical action of the solar rays on bodies is worthy of every attention on the part of the learned, by its influence on most of the phænomena of animated nature, and yet it has been hitherto but little attended to. M. Vogel has just added some experiments to those which we possessed on this head. Ammonia and phosphorus, which do not act upon each other in the dark, emit in the light phosphorated hydrogen gas, and deposit a black power composed of phosphorus and ammonia intimately combined. It is nearly the same case with phosphorus and potash. The action of the various rays is not always similar; the red rays do not produce any effect on a solution of corrosive sublimate in ether; whereas the blue rays and a full light effect a mutual decomposition. The highly oxidated metallic muriates are brought in the same way to the minimum of oxidation.

We said a few words the two last years on the researches of M. Chevreul Aide, naturalist to the Museum of Natural History, on soap and saponification. This accurate experimenter has ascertained that the action of potash produces between the elements of grease new modes of combination, from which result substances which did not exist completely formed before, and two of which, viz. margarine and a kind of oil or fluid grease, acquire all the properties of the acids. The author pursuing his labours, ascertained that the same effects are produced by soda, the alkaline and various metallic oxides, and that the substances resulting are in the same proportion whatever agent is made use of. Magnesia and alumine, on the contrary, confine themselves to contracting with fat a certain union, but without separating the elements into various compounds. The quantity of alkali necessary for converting into soap a given quantity of fat is precisely that which can saturate the margarine, and the oil which this fat produces. Our laborious chemist has terminated his memoirs on this subject, by assigning the capacity of saturation of the margarine and the greasy fluid, and by making known the properties of various new saponaceous combinations, which he produced by the play of double affinities, by mixing a hot solution of the fatty fluid and potash with various earthy or metallic salts. He thus succeeded in making the soaps, the study of which had been hitherto neglected, almost as well known as the salts, with which the chemists have been most of all occupied.

The late M. Fourcroy had made known under the name of *adipocire*, a substance which is separated by means of the acids from the fatty substance into which the bodies of animals buried in the earth are converted, and he regarded it as the same with that which is extracted in the crystalline state from the biliary calculi of men, and with spermaceti.

M. Chevreul, led by the course of his researches on fatty substances to examine these subjects, found that the adipocire of the biliary calculi gives no soap; whereas spermaceti supplies it equally with fat: but it is then a little deteriorated, and in other proportions, and with other properties. The fat of dead bodies is much more compounded than Fourcroy thought, and there have been found in it various fatty substances combined with ammonia, potash and lime. It is a fatty substance which has already undergone the action of the alkalies.

Every one must have observed a resinous excretion of an orange yellow colour which oozes from its fissures in the bark of the beech-tree when exposed to humidity, in the form of threads curled like vermicelli. M. Bidault de Villiers has made some chemical experiments on this substance. He dissolved one portion in water, another in alcohol, and the residue had several of the properties of gluten. The nitric acid converts it into oxalic acid, into a very abundant yellowish matter, and into a greasy substance; but it produces in it no mucous acid. It yields in the fire much carbonate of ammonia and a fetid oil: in short, it must be regarded as resembling, very closely, animal substances in its nature. It will be interesting to make some inquiries into the causes of its production.

One of the æras at which chemistry seemed to shine most brilliantly, and to be most useful, was that, without contradiction, when France, separated for twenty years from those countries the productions of which had become so long to us true wants, was obliged to supply their place by the productions of her own soil. The arts which were known have been perfected; new arts have been created. We have seen in succession soda extracted from sea salt; alum and copperas formed; colours rendered fixed, which were before supposed to be false tints; indigo supplied, and madder taking place of cochineal; while beet-root sugar was supplanting that obtained from the cane.

This last article, the most important of all, has not yet lost all its interest. It is true that many manufactories have ceased to exist, but those which have been conducted with intelligence and skill still subsist and prosper: and according to Count Chaptal, their products may always rival colonial sugars. This experienced chemist gives an irrefragable proof of his assertion, since he continues the manufacture with profit: it is true that in all the details of the cultivation, the harvesting and the preparation, as well as in the employment of the various kinds of refuse, he has taken advantage of the progress of science and experience, so far as not to reject any thing which can be o service, and to apply to other purposes whatever he has been obliged to reject. He has described his processes in a manner [illegible] clear, that they may be followed by all manufacturers, and we ought to hope that his work will assist in preserving to France a valuable branch of industry, and which a thousand events might render one of absolute necessity.

[To be continued.]

LXXXII. *Intelligence and Miscellaneous Articles.*

Letter from Sir George Cayley, *Bart.*

Green Hill, May 16, 1816.

Sir,—Since I forwarded my second paper to you on the subject of Aërial Navigation, I find from a paragraph in the Monthly Magazine that Messrs. S. I. Pauly civil engineer and Mr. D. Egg of No. 32, Strand, are preparing an experiment on the steerage of a balloon capable of carrying three or four persons. The great bulk inseparable from aërial vessels that are capable of carrying any considerable burthen with such speed as will render the invention valuable to mankind, is not to be expected from individual efforts; but I am glad to find professional engineers turning their attention to this subject; and should any efficient subscription take place, I hope the committee appointed to regulate it will encourage these exertions; and that by the aid of these gentlemen, in conjunction with the advice of the first civil engineers the country affords, experiments upon the most efficient scale may soon lead to the establishment of the art.

I remain, sir,
Your obliged and obedient servant,
Geo. Cayley.

M. de la Roche, in a memoir on the propagation of sound printed in a late number of the *Annales de Chimie et de Physique,* draws the following conclusions:

1. That the wind exercises almost no sensible influence on sounds heard at short distances, for instance six metres. 2. When the distance is greater, the sound is heard far less in the direction contrary to that of the wind, than in that of the wind itself. The difference seems to be the greater, in proportion as the distance itself is greater.

From the approximation of those two propositions, M. de la Roche thinks it results, 1. That the law of decrement of sound is not the same in the direction of the wind as in that which is opposite to it. 2. That the influence of the wind on sound is not more exercised on the spot where it has been produced, than on every particle of ground which it passes over. 3. That sound is heard a little better in a direction perpendicular to that of the wind, than in the direction of the wind. 4. That ca[illegible] foreign to the wind, and depending on modifications of the atmosphere, have a very great influence on the facility with which sound is propagated to a distance.

MUNGO PARK.

Every circumstance that can elucidate the fate of the explorer of Africa is interesting, although, from the length of time that has elapsed since he was last heard of, the probability of his being in existence is so chimerical as scarcely to afford the most distant ray of its being realised. The following is, however, a singular coincidence, and there can be no doubt but the white men therein mentioned might be Mr. Park, and probably his fellow traveller Lieutenant Martyn; particularly as it is known they embarked on the Niger with only three of their companions, and also that no persons, as is there described, could possibly be in the interior of that country, and similarly situated, but them. The manner in which this information has been obtained is rather singular, but there seems no reason to doubt of its correctness. It appears that some time since a gentleman accidentally observed in London a seaman whom he had seen at Cadiz, where he was well known from his having been in Africa, and at Tombuctoo, a city which no white person before has been able to reach, although it is the great object of European research. This seaman, whose name is Robert Adams, belonged to the American ship Charles (and is a native of America, born on Hudson's river), and was wrecked October 11, 1810, near a small place called El Gazie, on the African coast, to the northward of Cape Blanco; and, with the rest of the crew, made prisoner by the Moors. After some time the whole were conveyed by the barbarians across the Great Desert to Soudenny, and thence to Tombuctoo, experiencing the greatest fatigue and hardships. After a slavery at various places for four years, and undergoing every cruel treatment, he was so fortunate as to have his ransom effected by Mr. Dupuis, the Consul at Mogadore, from whence he went to Fez, obtained a passage to Cadiz, where he remained until peace with the United States was concluded, and ultimately arrived in London. He states, that among the negro-slaves at Wed-noon—(where, from his being a white man, he attracted great notice)—was a woman who said she came from a place called Kanno, a long way across the Desert, and that she had seen in her own country white men as white as "buther" (meaning the white wall), and in a large boat with two high sticks in it, with cloth upon them; and that they rowed this boat in a manner different from the custom of the negroes, who use paddles. In stating this, she made the motion of rowing with oars, so as to leave no doubt that she had seen a vessel in the European fashion, and manned by white people.—Adams arrived at this place August 23, 1812, and remained there till September 1813.

Many of the slaves purchased at Tombuctoo and other places, and brought by the Moors and Arabs across the Great Desert, come from countries very far to the eastward; it is therefore not improbable to suppose that the place from whence this woman came might be the kingdom of Ghana, or Cano, on the Niger, lying between the 10th and 15th degrees of east longitude. Supposing this correct, the curious relation of this person will afford reasonable ground for conjecturing that Mr. Park had made further progress on the Niger than where the guide states he parted with him; and as Mr. Park's death was by drowning, together with his companions, and only occurred the day after the guide gave up his charge, he could not have made any great progress in his voyage. The time that intervened between the departure of Mr. Park from Sansanding, where he embarked on the Niger the 17th of November 1805, and his reported death at Silla, either in March or early in April 1806, would greatly admit of his having reached a territory more distant than Kanno. That this enterprising traveller has met his death is almost certain; but the time, place, or circumstances under which it occurred, are enveloped in mystery, and rest alone on the bare assertion of his guide, who, it must be recollected, was not an eye-witness of the event, but obtained the information from others.

At Wed-noon, the only white person whom Adams found there was a Frenchman, who had been shipwrecked and taken into slavery. The temptation which had been held out to this man, as is invariably done by the followers of Mahomet to all Christians who unfortunately fall into their power, was too strong for him to resist. He had therefore turned Mahometan, and was in consequence circumcised; by which means he was immediately removed from slavery, allowed to marry, and was the father of several children. At this place he had resided twelve years, and obtained a livelihood by making gunpowder, which was purchased with great avidity by the Moors and Arabs, all of whom were entirely ignorant of the process used in its manufacture. This secret the Frenchman studiously kept from them, and always made the gunpowder alone in a room of his house, to which no one else had access. It is reported that he is since dead.

STEAM

STEAM ENGINES IN CORNWALL.

The average work of thirty-one steam-engines in the month of April, according to Messrs. Leans' Report, was 20,998,138 pounds lifted one foot high with each bushel of coals. Woolf's engine at Wheal Var lifted 43,998,178 pounds, and his engine at Wheal Abraham 50,908,433 pounds with each bushel.

FULMINATING PLATINA.

Mr. Davy, professor of chemistry to the Cork Institution, has lately discovered a new combination of platina, which belongs to the class of fulminating metallic compounds, and has some curious properties. It explodes by a moderate degree of heat; the explosion is accompanied by a flash of light; and the products are gases and metallic platina. In ammoniacal gas, at the common temperature of the atmosphere, this substance becomes ignited; and in liquid ammonia, gas is copiously evolved from it. When it is touched with alcohol, a slight crackling noise is produced, and it burns with a red flame Mr. Davy is engaged in the examination of this compound, and will shortly make known the results of his investigation.

LECTURE.

Theatre of Anatomy.—Mr. Taunton, F.A.S. Member of the Royal College of Surgeons of London, Surgeon to the City and Finsbury Dispensaries, City of London Truss Society, &c., commenced his Lectures on Anatomy, Physiology, Pathology, and Surgery, on Saturday May 18, to be continued every Tuesday, Thursday, and Saturday, at Eight o'Clock in the Evening *precisely.*

Particulars may be had on applying to Mr. Taunton, 87, Hatton Garden.

Meteorological Observations made in Scotland, principally at Edinburgh, from the 14th to the 25th of April 1816.

April 14.—Fine cold day, with occasional showers of small snow and sleet. Wind from NW. The sea-gulls flew low about the rocks below West Leith. Night bright and clear.

April 15.—Clouded, and not so cold as yesterday, with more rain. Dark night.

April 16.—Clouded early; fair day, with wanecloud, par-

taking

[illegible] of the cirrocumulative and flimsy form in transient beds, P.M. The wind became high at night*.

April 17.—Being early this morning on the high point of ground called King Arthur's Seat, I noticed the formation of stackenclouds around me, while waneclouds in detachments appeared above, with some curlcloud. During the day large twaincloud was formed with cirrous crowns ending in raincloud pouring light hail and sleet. There is as yet scarcely any appearance of spring here; except the flower of the *Crocus vernalis*.

April 18.—Obscure and cold, with snow all the afternoon. The inhabitants of the city of Edinburgh complain of an unusually protracted winter.

April 19.—Clear morning; fragments of loose stackencloud flying along in the wind. The lighter modifications in a higher atmosphere, but of confused and indefinite character.

April 20.—Windy unpleasant day. A fine afternoon.

April 21.—Fair warm day and calm, an evident change in the atmosphere, abundance of curlcloud of varied forms, wanecloud, sondercloud of indefinite character, and stackencloud in small quantity. Obscure horizon. Dark blue coloured sea, off Leith coast. Fine spring evening, with yellow polestreamers, or northern lights. No leaves on the trees here yet. Waneclouds by night.

April 22.—Fine warm morning; various clouds; small rain P.M.

April 23.—Chiefly clouded over, and cooler, with a breeze. Dark night.

April 24.—This morning being out early, I noticed that the tops of the hills were covered with fog, while their bases and the town below were clear. This cloud did not appear to be fallcloud, but wanecloud. The latter is often a ground cloud like the former, and it constitutes the wet as distinguished from the dry mists. Cloudy day with small rain, such as is called a Scotch mist.

April 25.—The same sort of mist and rain as yesterday.

6, College-Street, Edinburgh,
April, 26th, 1816.

THOMAS FORSTER.

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* The season has certainly been unusually backward both in Scotland and England, and the weather particularly changeable. As popular clamour deduces an unusual mortality, particularly of sudden deaths, I am induced to request the communication of facts on this subject from your medical correspondents in different parts of the country, and also wish to know whether, in districts where it has most prevailed, any accurate observations have been made on any peculiarities of the weather.

METEOROLOGICAL JOURNAL KEPT AT BOSTON, LINCOLNSHIRE.

[The time of observation, unless otherwise stated, is at 1 P.M.]

	Age of the Moon	Thermometer.	Barometer	State of the Weather.	Modification of the Clouds.
1816.					
April 16	19	50 0	29 61	cloudy	heavy rain the previous night
17	20	48 0	29 59	fair	
18	21	52·0	29·62	very fine	
19	22	50 0	29 90	do	
20	23	55 0	30·20	do	
21	24	50 0	29 90	do	
22	25	56 0	29 90	rain	
23	26	53 0	29 95	fair	
24	27	54 0	30 05	very fine	
25	28	58 0	30 15	do	
26	29	58 0	30 25	do	
27	new	56·0	30·10	do	
28	1	55 0	29 90	do	
29	2	62 0	29 70	fair	
30	3	59 0	29·72	rain	
May 1	4	53 0	29 80	fair	
2	5	63 0	29 69	violent thunder-storm with very heavy hail and rain	
3	6	55·0	30·0	rain	
4	7	55·0	30·15	fair	
5	8	53 0	29 95	rain	
6	9	54·0	30 0	do	
7	10	55·0	29·94	do	
8	11	56 0	29·63	do	
9	12	50·0	29·60	do	
10	13	43·0	29 45	do	
11	full	42·5	29·50	do	
12	15	41·0	29·35	violent hail-storm	
13	16	47 0	29·70	slight showers	
14	17	56·0	29 97	very fine	

METEORO-

METEOROLOGICAL TABLE,

BY MR. CARY, OF THE STRAND,

For May 1816.

Days of Month.	Thermometer. 8 o'Clock, Morning.	Noon.	11 o'Clock, Night.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
April 27	47	66	50	30·02	62	Fair
28	50	67	52	29·84	60	Fair
29	57	61	55	·55	41	Cloudy
30	54	60	51	·56	49	Fair
May 1	47	55	46	·62	63	Fair
2	52	55	47	·80	60	Fair
3	52	55	50	·98	0	Rain
4	50	52	50	30·03	0	Rain
5	50	51	49	29·80	0	Rain
6	49	55	48	·95	36	Cloudy
7	50	54	50	·70	49	Showery
8	50	52	48	·50	0	Rain
9	47	52	46	·47	0	Rain
10	47	47	45	·20	0	Rain
11	43	47	40	·38	37	Cloudy
12	40	45	37	·39	30	Sleet showers
13	44	47	43	·67	27	Showery
14	47	60	45	·82	34	Fair
15	52	55	55	·82	0	Rain
16	52	67	55	·83	29	Fair
17	57	66	47	·79	44	Fair
18	46	54	47	·81	40	Cloudy
19	47	50	47	·85	35	Cloudy
20	48	57	48	·90	47	Fair
21	48	61	46	·85	45	Fair
22	49	66	49	·84	50	Fair
23	50	66	49	·84	39	Fair
24	51	55	51	·83	34	Foggy
25	55	57	48	.82	29	Cloudy
26	50	63	50	30·12	61	Fair

N.B. The Barometer's height is taken at one o'clock.

LXXXIII. *Some Account of the Monuments of Thebes in Egypt.*

[Continued from p. 385.]

Of all the portions of the edifices of Medynat-Abou, the peristyle in the midst of which we are is indisputably that which strikes most, by its imposing mass and character of grandeur: we are convinced that its founders wished to render it indestructible, and that the Egyptian architects charged with its construction have strained every nerve to make this monument pass down to the most distant posterity. The elegance of its columns cannot, it is true, be boasted of, but they are colossal; they are nearly seven feet and a half in diameter, and do not appear to be too bulky to carry the enormous stones which form the architraves and the roofs. When we wish to give an account of the sentiment of admiration which we experience at the sight of this edifice, we find that we are particularly led away by the grandeur of those lines which throughout a long space present no interruption, and the perfect execution of which answers to the magnificent manner in which they have been conceived. If our architects had not already returned to sound principles, they would find here a proof that irregular or *tormented* lines and projecting bodies can never be in architecture the source of any species of grandeur and beauty. But what adds much to the effect produced by the peristyle are the caryatid pilasters which decorate it. "How, in fact, can we refrain from being seized with a religious and profound respect at the sight of that council of gods united, in some measure, to dictate the laws of wisdom and philanthropy which we see every where written on the walls of the palace! The Egyptian artists, in throwing these statues of the gods against pillars which support rich roofs decorated with stars of a golden yellow colour scattered over a blue ground, seem to have wished to represent to us the supreme Being under the azure vault which he fills with his immensity! How profound must have been the impression produced by the sight of this place on the ancient Egyptians, with whom every thing had a mystical and religious meaning, if we who are strangers to their manners, customs and worship, cannot penetrate without emotion into the midst of these galleries, every supporter of which is a god! How nobly monumental is the simplicity of the attitudes and the form of the statues, and how greatly does the rigid immoveability add to the imposing aspect of the whole edifice! What a superficial examination might induce us to regard as the infancy of the art, seems on the contrary to be the result of a foreseen and calculated perfection."

I have given the above passage from the *Description de l'Egypte* entire, and I shall give others still, because the mere expression of the effects of these monuments on the observer may suggest in the imagination of the reader ideas which the simple indication of these dead masses could not possibly give. The northern part of the palace is almost wholly in ruins; nevertheless we can distinguish several chambers which appear to have been inhabited.

What is most remarkable in those ruins is the sculpture with which the external and internal surfaces of this palace are covered. Those outside are historical: they are generally battles or sea-fights. In the land battles there are several in which the victory is on the side of the Egyptians. The chief or king always appears in a car; he is of a colossal size, and armed with a lance and bow and arrows. The wheels of his chariot break the enemy's ranks. The Egyptians are partly in the midst of the fight: other columns also come up in succession. This same figure of the king reappears several times: sometimes he marches slowly and stops, and sometimes he pushes his coursers into the midst of his enemies. In other parts there is a lion hunt described. The king in his car pursues into the thickest part of the wood two lions, one of which is just expiring; the other, which still flies, is struck by four arrows. The most remarkable of these representations, however, is the naval combat. It represents an invasion repelled, in which the victory has already declared for the Egyptians. The king is placed on the shore; under his feet are enemies biting the dust; there are others before him;—he looks sternly at the enemy. Quite close to the shore is the battle of the two fleets. The Egyptian vessels, entirely different from those of the Nile (they may well be called long vessels,) have their prows shaped like lions' heads, those of the enemy are of similar construction. The combat is evidently not terminated, but victory will decide for the Egyptians. Some of the enemy's ships are entangled, others are taken, some are sinking. We see in these representations the rudiments of naval tactics. The enemy's squadron is surrounded by that of the Egyptians', and apparently there is no chance of its escaping. In all these hostile pictures the different nations are carefully distinguished from each other by their dress and armour. In the land combats the enemy have always the beard and the tunic long. In the naval combat they wear short and light habits: part of them wear a bonnet, like the caps of modern soldiers, and surmounted with feathers; others have leathern helmets.

By these traits we recognise a southern nation; and the French artists have taken them for Indians. This variety of costume is also religiously observed in the pictures which form the rest of the series. But the greater part of the building being in ruins, these representations are in bad preservation; besides, there have been no drawings of them yet taken.

The sculptures of the interior of the palace are of a different kind, but they are still analogous. They consist of triumphal marches intimately connected with religious ceremonies: not only are the processions directed towards the gods, but the gods themselves take part in them. The most curious of these bas-reliefs are to be seen on the fine peristyle which we have mentioned. One of the walls of this part of the palace represents the victorious king stopping in his chariot; we recognise his royalty by the serpent on his head. The horses adorned by rich coverings are caressed by grooms: the king in a majestic attitude seems to turn round and desire the prisoners to be brought to him. The latter arrive by threes and fours at a time, led by an Egyptian. This march is represented by the artists in four rows raised above each other. The prisoners are enveloped in cloaks of a blue and green colour, under which they have still another covering. The Egyptians have white vestments with red streaks: the colours have preserved all their brilliancy. The prisoners are without arms—the hands are in different attitudes, partly tied over the head. In front of the car of the conqueror are the hands and parts of generation of those who have perished: the prisoners brought in are not mutilated.

On the northern wall of this peristyle is represented a triumphal march. The king seated on his throne is carried by eight warriors in a rich palanquin: these warriors are decorated with feathers, the emblems of victory. The throne is covered with a superb carpet; the feet of the conqueror rest on a cushion: he bears in his hand the cross and the keys, the attributes of divinity; two genii placed behind him cover him with their wings. The lion, the stag, the serpent, and the sphinx, the symbols of his grandeur, are by his side. The procession is composed of warriors decorated with palms and feathers, and of priests who offer up incense. A priest with a scroll in his hand seems to proclaim the exploits of the conqueror. The cavalcade proceeds towards the temple of Osiris, whose statue is to be seen. Four priests go before the hero to conduct him into the temple, where he is to offer his sacrifices. Soon the march is continued: the god himself, quitting his sacred residence, accompanies the king, and twenty-four priests bear the god under a magnificent canopy. The conqueror, who has changed his clothes and head-dress, opens the march. Above him flies the stag, and the sacred bull accompanies the procession. In front of it march seventeen priests bearing the attributes of the divinity. In general this march has become a religious procession: the priests, and no longer the warriors, are now the principal personages. Soon again the scene changes entirely, and the king performs the functions of sacrificing high-priest. It is remarkable that this scene seems to relate to agriculture. A priest presents to the king a handful of stalks which he cuts with a hook: afterwards he offers his gifts to the divinity. We ought to separate this scene entirely from the preceding one, and represent the king protecting the arts of peace, as the former shows him in his military glory. This ænigma would not be one to us without doubt if the whole of the sculptures had been preserved.

One of the lateral halls also contains some curious sculptures: they seem to represent the initiation of the king into the mysteries.

The royal candidate is purified in the first place by some priests. Others take him by the hand to introduce him into the sanctuary. Here every thing is mysterious. The priests are almost all covered with masks of animals.

In all these representations the French authors see the exploits of Sesostris, to whom, in fact, we may ascribe this palace.

At some distance from the palace, to the north-west, is the Temple of Medynat-Abou, which fronts the Nile. This temple has buttresses which are not finished, and which seem to be of an origin posterior to that of the principal temple. It is almost entirely in ruins; the distribution of its parts is in conformity with that of the other temples.

To the north-west of this temple a plain extends which is extremely curious: it is partly covered with a forest of *Mimosa*, and bears the name of the field of the Colossi. Not less than seventeen colossal figures are counted in it: some are in an erect posture, others are only half erect, and some are completely thrown down. Among them is the famous colossus of Memnon, which at sun-rise emitted certain sounds.

In the first place, strangers remark two colossi joined together, now called the one on the north *Thama*, and the other *Chama* on the south; both of them front the Nile. They are both of free-stone, are forty-eight feet high without the pedestal,—in all sixty. The weight of each when whole is estimated at 2,612,000 lbs. The Chama is in one piece; the upper part of the other is composed of five pieces. As all the colossi of the Egyptians were monoliths, it cannot be doubted that the Thama was so originally. It is this same statue which, from the interpretation of the inscriptions which it received in the two first centuries of the Christian era, was regarded as that of Memnon; for the inscriptions say that he who engraved them had heard the sound of the colossus. Doubts, however, have been started of this fact, founded partly on the quality and colours of the stone*, partly on a passage of Strabo, in which it is said that in his time the stone was broken in the middle, which is confirmed by the description of Pausanias. It is added also, that it is unknown at what period the colossus must have been repaired. All these doubts, however, cannot have much weight. The stone is in fact, according to the representation of the French authors, a free-stone blackened by the influence of the air: and because we are ignorant who repaired the statue, we cannot reject the evidence that it has been so repaired. If we might hazard an hypothesis, might we not suppose that the reign of Septimus Severus, who made so many reparations of monuments in Egypt, was the era?

Some distance from thence, to the N.W., we are struck with two immense moles of stones loaded with hieroglyphics; they have probably served as pedestals for two other colossi. More to the northward, beside a triple row of columns we find a considerable fragment of a colossus in the attitude of walking, thirty feet high;—a little further on there is the trunk of a statue seated, of black granite. To the north are the remains of a column of yellow marble, marching;—further on are the ruins of two others of red granite, seated;—two others succeed, forty feet high each, marching. In general, if it is now proved that since the commencement of our era the soil has been raised from 15 to 20 feet on this point, how many broken or overthrown statues may be still concealed under ground!

How came this quantity of colossi to be heaped up in this apparent disorder? This is the first question which occurs. The appearance of the place, the ruins of the columns scattered here and there, give reason to suppose that there was an immense building formerly there, the whole length of which, with the pylones, the courts, the galleries and apartments, could not be less than 1800 feet. It is apparently before the pylones and before the entrances of the courts and porticos that these colossi were placed: we see this at least in the Palace of Osymandyas, and others. In general it was not customary in Egypt to place colossi anywhere else than within or in front of the buildings. As to the sphinxes which formed alleys, it was certainly otherwise. But what confirms our opinion is, that Strabo and Pliny place the colossus of Memnon in an edifice which the former calls the Memnonium, and which the other calls the Serapeum. Nevertheless, if the enormous dimensions which a building must have had adorned by such colossi, excite astonishment, it appears no less extraordinary that such slight remains of it are to be found. This new doubt will vanish of itself, if we suppose that the building was of limestone; and nothing contradicts this opinion, for in fact the materials of those buildings have since been used for lime. Their number must have been considerable; the immense excavations in the calcareous rocks prove this. There likewise remains near this colossus the remains of an ancient building constructed of this stone.

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* Pococke and Norden differ in opinion respecting the statue of Memnon. Pococke, vol. ii. p. 101, considers it to be the colossus which we have described. Norden, t. ii. p. 128, (edition of M. Langles) seeks for it in a colossus which is to be found in front of the Temple of Osymandyas, and which is broken in the middle. Count Veltheim has defended the opinion of Norden in his *Antiquarische Aufsaetze*, t. ii. p. 69; but his reasons are not very plausible. The inscriptions on the colossus of Pococke evidently prove that at the period when they were made this colossus was regarded as that of Memnon. Is it probable that without some reason the tradition has been transferred from one statue to another? The opinion of Norden is supported also by that of M. Langles.

It is also more to the northward that the building is to be found which modern travellers, and particularly Norden, generally call Memnonium*, but which must be designated the Palace and Tomb of Osymandyas. The ruins of this building are among the most picturesque of those of ancient Thebes. This palace was built of free-stone. The pylones, many of the columns and caryatid pillars of this building, are still standing, while the ruins of so many others are heaped up around in hillocks. Here also we enter by a superb gateway into a square court, which is upwards of 140 feet long by 161 broad. It is in ruins, with the exception of two columns still standing, and so encumbered with blocks of granite as to resemble a quarry more than a court. By and by we discover the ruins of a formidable colossus destroyed with violence, but of which the head with a foot and a hand still remain. The fore finger is nearly four feet long, the distance from one shoulder to the other in a straight line is twenty-one feet. The height of the whole could not have been less than forty-five feet. The pedestal, eighteen feet high, is still standing opposite the second gateway. The pedestal, like the colossus itself, is of the finest red granite. It is near this city also that we still find the spot from which this mass of two millions of pounds has been detached, in order to be conveyed to the place which it afterwards occupied, forty-five leagues off. The researches made on the spot have [illegible] that this building contained four similar colossi, one of which of granite seems to have been placed beside that just mentioned.

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* In order to avoid all confusion in the topography of Thebes, it must be observed that Norden and other travellers call Memnonium or Palace of Memnon, that which with more reason ought to be called the Palace of Osymandyas. Pococke falls into another error, in taking for the Memnonium the Palace of Medynat-Abou. Sir W. Hamilton is also of this opinion. Nevertheless it is between these two buildings that the building in ruins is to be found, to which the statue of Memnon belonged, and to which Strabo has given the name of *Memnonium*.

By a second buttress, less elevated than the former, we enter into a peristyle which is also 140 feet long by 160 broad. This peristyle was surrounded by galleries formed to the north and the south by a double row of columns; on the east by a single row of caryatid pillars, and on the west by columns and caryatid pillars. The southern part is demolished; but the northern part is in sufficient preservation to enable us to judge of the whole with some certainty. This peristyle also contains two colossi, each about twenty-three feet high. One is entirely of black granite; the other has also a black body, but the head is of red granite. This head is preserved. It has a calmness full of grace, and that happy physiognomy which more than beauty itself has the art of pleasing; the corners of the mouth, a little raised towards the eye, express a smile. We cannot represent a divinity under traits which can make it more venerated.

From the peristyle we enter by three doors into a vast apartment, the roof of which is supported by sixty columns in ten rows, each containing six columns; four of those columns are still standing, but dispersed. This hall was divided into three compartments. We may conceive an idea of the majesty of the whole, when we recollect that the columns of the middle compartment, greater than the rest, are thirty-five feet high, and more than six feet in diameter. Adjoining this hall is a second and a third, and in each are eight columns still standing, and of the same size.

Such are the remains of a building which, great as it now appears, must have been formerly still more extensive. If it excite admiration as a monument of architecture, it is not less remarkable on account of the sculptures which cover its walls. The latter are still partly covered with sacred hieroglyphics, and partly historical bas-reliefs. The former represent, as usual, divinities, and the offerings which are made to them; but the bas-reliefs deserve a particular examination. Unfortunately it is the same with them as with the whole building—a small part only remains.

The first of these bas-reliefs is on the interior surface of the first of the two grand buttresses. It is a representation of a battle—The infantry advance in close column: at the head is the chief, of a colossal height, in a chariot. Further off is seen the tumult of the battle. The chiefs in their chariots precipitate themselves on the enemy. Dead, wounded, and dying—men and horses flying—all is confusion. In the middle of the field of battle a river is seen, into which the fugitives throw themselves, while their countrymen are ready to receive them on the oppositive bank. On the left side of the buttress a hero is seen of colossal stature seated on a throne elegantly adorned. His feet rest on a footstool on which captives are represented. The cushions of the seat and of the footstool seem to be precious stuffs studded with stars. A procession of twenty-one persons in long vestments approach him in a respectful and suppliant posture. Hard by we see chariots and warriors with immense bucklers. The army to which they seem to belong has a rear-guard of infantry and chariots, each carrying a warrior. The baggage is assailed by the enemy, but vigorously defended. The walls of the peristyle exhibit representations not less interesting. There is a combat there also. It is a hostile invasion repulsed. A river traverses the field of battle, forming numerous sinuosities. In several places we see the remains of the blue colour with which the river was painted. It waters a fort, the object of the movements on both banks. The inhabitants of the fort, however, have passed the river. They have beards and long tunics: three are in each chariot.

The Egyptians, on foot or mounted in chariots, are commanded by their king and divided into detachments, each having its head or chief of a higher stature than the rest. They overwhelm every thing which they meet on their passage, and trample under foot the dead and dying. Numbers of the enemy are drowned in trying to pass the river. They are pursued by the victors.

On the walls of the great hall is represented an assault, and at the same time the taking of a fort, perhaps the continuation of the foregoing battle. At the foot of the wall we see a kind of tortoise formed by large bucklers. Behind, and partly under this machine, are the warriors, whose feet only are uncovered. A ladder is applied: soldiers ascend it. Already is the first of the four approaches to the fort gained; the battle continues: the besieged throw down stones and burning substances. But the issue of the combat is no longer doubtful; and the flag which is seen, is perhaps, although pierced with arrows, the signal that they wish to surrender. It is to be presumed that the parts of the palace which are destroyed, represented the triumph of the conqueror: and if this palace be in fact that of Osymandyas, described by Diodorus, we should have found a still more interesting scene,—the supreme tribunal of Egypt, and the grand judge having the symbol of truth on his breast.

The western district between those grand buildings and the Libyan chain is not without its monuments. We there see a Temple of Isis, smaller but curious and well preserved. It is here particularly that we remark in all its lustre the play of colouring with which the bas-reliefs are adorned. The dimensions of this building admit of our taking in the whole at a single glance, and of judging with more certainty of the effect produced by those ornaments. M. Denon takes this occasion to remark, that "in this case the union of sculpture and painting, which might appear strange, presents nothing disgusting at first sight. The eye is rather pleased with the sensations which it excites."

The bas-reliefs refer to religious subjects: the most curious of the whole is a judgement of the dead, as it is painted on several monuments*. It is very probable that this temple served at the same time for a burial-place.

On proceeding from this monument of the palace of Osymandyas northward, we find ourselves in the midst of an alley of pedestals which is only interrupted to begin once more. Rigorous inquiries have proved that this was an alley of sphinxes to the number of two hundred, all of colossal size, if we may judge by the pedestals, which are six feet broad by twelve long. The breadth of the alley was forty feet, the distance of the statues seven. What must have been the size of the edifice to which this alley led! We find enormous ruins of buttresses, walls, and stair-cases; but nothing entire. What is also remarkable is a building which presents the form of a vault, without there being one actually, as the most scrutinous inquiries have demonstrated; a new proof that arches were entirely unknown to the Egyptians.

There still remains on this side of the Nile the building northwest of Thebes, near the village of Kurnu, the name of which it also bears. The Palace of Kurnu, (*El Gurnu* according to Sir W. Hamilton,) although not among the number of the magnificent monuments of this ancient residence, is nevertheless too large to entitle us to suppose that it was the residence of an individual. This monument is the more remarkable (not being a temple), as it seems to offer a medium between the vast palaces of the kings and the houses of individuals. We find here neither sphinxes, nor obelisks, nor those enormous buttresses, nor a colonnade: all seems destined for a residence. If the whole is not colossal, it is nevertheless very large. A portico 150 feet long, supported by ten columns, forms the entrance: this is preserved almost entire. Two door-ways lead from the porticos into the interior of the building. By the middle door-way, which is the largest, we enter a vestibule supported by six columns; beyond this several doors open from the halls and apartments. The door-way of the portico on the left also leads into a hall, beside which are several other halls, and further on some smaller apartments. The right side seems to have been arranged in the same way, but the whole of it is in ruins. The whole building was therefore composed of three parts, independent of each other, but united by the grand entrance portico. This palace differs from the rest also in so far as it is without those historical or religious representations which cover their walls. We must conclude, however, that if it has not been the residence of kings, it must be at least admitted that it was inhabited by some grandee of the empire *.

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* In the *Idées sur la Politique des Peuples de l'Afrique*, t. ii. p. 655, we find this passage: "We see in this picture the god Osiris seated, judging the dead. He is known by his ordinary attributes. Before him is the flower of the lotus, the symbol of life eternal, and a lion apparently as if guarding the infernal regions. A small figure is placed in a large balance by two genii with the heads of animals; the one has the head of a dog, the symbol of gross sensuality; the other has the head of a stag, the common symbol of purity, religious and moral. The two genii raise their hands to the balance, and seem to make representations to Osiris. Before this god is Hermes with a head of Ibis, with tablets in his hand to mark the virtues and vices of the defunct."

[To be continued.]

LXXXIV. *On the Principles of Security in Sir* HUMPHRY DAVY'S *Lamp. By* J. MURRAY, *Esq.*

To Mr. Tilloch.

SIR, — IN page 319 of the Annals of Philosophy, an anonymous correspondent attacks the security of Sir H. Davy's safe-lamp: 1. Because the Philosophical Magazine *approves*; and, 2. Because the *principle* on which its safety rests has *not been developed*. You will, sir, no doubt, allow this writer whatever benefit his objections may claim from the first. I shall endeavour to relieve the second ground of his scruples, premising that it appears to me unwise and unbecoming to indulge in scepticism because the principles of the phænomenon are veiled from ken. In the needle, the polarity is palpable to sense—the cause *unknown*. In the case of this curious instrument, has it not undergone "the torture of the fire?" I lament to see the remarks of Dr. Reid Clanny, and the mode he has adopted in introducing himself to public notice—whatever analogy may obtain in the lamp constructed by Stephenson and the *first* projection of Davy, there is no semblance whatever between the lamp of Dr. Reid Clanny and the latter.

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* For the beautiful engraving of this building which accompanies the present number, we are indebted to the kindness of Mr. C. Taylor, of Hatton Garden. It is one of the many elegant plates which embellish the work entitled EGYPT, now publishing by Mr. Taylor.

When a bar of metal, &c. is plunged into flame, there is a *chasm* around the cylinder of about one-sixteenth of an inch width, and it is important to remark that this distance is maintained, *however unequal* the diameter of the rod may be—a mass several inches thick, and wire of the most delicate dimensions, equally repel the surface of flame. Hence the plexus of wire has no reference to the *magnitude* of the metal employed in its fabrication. There will, therefore, be no danger, in wire of a proper thickness, of the meshes being dissolved by the action of flame: besides, the lamp will only be subject to the continued attack of the flame of fire-damp for *a few seconds* occasionally. In an atmosphere which has reached the maximum, the lamp is *extinguished*; and where the carbonated hydrogen mixes only in minute proportion, the flame of the lamp is magnified to an extent which is insufficient to fill up the cylinder, or approach nearly to the interior surface of the sides of the wire-gauze. Parallel bars, I found, prevented the communication of flame with equal facility as wire-gauze. The interval must not exceed one-eighth of an inch. Here, therefore, is a limit pointed out to us:—If the meshes are one-sixteenth of an inch apart, no danger would occur should an accident break down the *alternate* one. When the bars are crossed by others at right angles, it constitutes a *double* security. The phænomena described are supported when the wire is *red hot*. The phænomenon is not *magnetic*, for copper, zinc, and silver, ward off the flame as well as iron and nickel; nor is it connected with *electrics* and *non-electrics*, for bars of glass, &c. serve the purpose as well as rods of metal. A concave surface of silver attracts flame, but still maintains the "*appointed*" distance. Is this interval filled up with *caloric*, distinct from the combustion? We may thrust a match into a cone of flame, and it is only burnt exterior to the surface, where its temperature appears to be exalted to ignition. Were it simply an envelope of emanating heated agate and carbonic acid gas, combustion would be prevented. Flame will not kindle gunpowder; and, on the contrary, inflammable gases are not acted upon by the spark, unless exalted into at least incipient flame. This remarkable fact merits investigation.

I made sieves of hair, whale-bone, &c. and found them secure as metallic wire.

The lamp of Sir H. Davy is indeed "a present from philosophy to the arts," and its distinguished author "well merits the civic crown." Science never shines with a sublimer lustre than when regarding the interests of humanity.

I am respectfully, sir,
Your very humble servant,
J. MURRAY.

Liverpool, June 5, 1816.

LXXXV. *Some Account of the new Hot and Cold Baths at Ramsgate.* *By* A Correspondent.

It has been remarked by intelligent travellers, that of all the situations on the coast of Great Britain, frequented for the purpose of sea-bathing, no where are the two great requisites of salubrity of climate and picturesqueness of scenery more strikingly united than in the isle of Thanet. The beautiful district of rising ground known by that name, is the most eastern point of land in England, and consists of a solid mass of chalk shelving into the German Ocean, above the level of which it is considerably raised, forming a romantic peninsula, having four-fifths of its boundary washed by the sea.

In consequence of the immense beds of calcareous matter which compose this peninsula, the air is remarkably pure and free from moisture; as an effect of the rapid absorption of humidity by the soil, no stagnant water is to be met with, and the rain which falls disappears so rapidly, that the most delicate invalid may walk out with perfect safety immediately after the most copious shower.

The town of Ramsgate, which stands on the southern side of the island, enjoys all these advantages in a very eminent degree. From being built in a valley formed by an indentation in the cliff, somewhat in the manner of an amphitheatre, open towards the sea, it is completely screened from the cold winds to which the towns on the opposite side are exposed. Its sheltered situation promotes that luxuriant vegetation in the town and its immediate neighbourhood, which forms a striking contrast to the bleak and monotonous scenery of other bathing places.

Notwithstanding the great number of persons, however, who annually visit Ramsgate for bathing and recreation, in consequence of its excellent beach and various other attractions, no establishment on a scale adequate to the rising importance of the place, has been hitherto formed for hot sea-water bathing. A building, from the designs of Mr. Meikleham, has therefore been lately erected at Ramsgate for warm sea-water bathing, which, from the scientific nature of its construction and arrangement, promises to be one of the greatest acquisitions to the public, since the introduction of the warm baths into England.

The new baths are situated on the west cliff at Ramsgate, and at an elevation of one hundred and ten feet above the level of the sea: they range with the other buildings of the Paragon, having the space between the front and the cliff laid out as a promenade. (Plate III.)

A horizontal tunnel has been excavated in the rock, running at the level of low water under the building, to a distance of one hundred and eighty feet, until it joins with a vertical tunnel from the top. The tide ebbs and flows in this horizontal tunnel; and the pumps in the vertical one, are so placed as always to get their supply at high water. In order to have the supply as free from vegetable impurities as possible, a wooden trunk has been carried from the entrance of the tunnel to a distance of one hundred and five feet into the sea; having gratings at small distances to intercept the weed, which might be driven into the pumps by the violence of the tide. This tunnel has a sluice at its upper extremity for the purpose of clearing out the sand which may subside at the bottom by means of a back current.

The pumps are worked by horses; and the water after being raised 110 feet, flows into a large reservoir, from which it is conveyed through pipes into the boilers and other parts of the building.

The building which contains the baths is 120 feet long and 34 feet deep, fronting the sea. This building is divided into three parts, viz. a circular centre, with the baths and dressing-rooms in each wing; the front is finished with handsome stone ornaments. Two flights of steps conduct into the saloon finished with pilasters at equal distances on its circumference, and is furnished with the daily papers, reviews, and other periodical publications for the accommodation of those who use the baths, and the floor is five feet above the level of the promenade for the advantage of a more extended prospect, and surmounted with an elegant ornamented dome-ceiling. The semicircumference of this room is open towards the sea; and a person seated in the centre of it commands one of the most extensive and varied prospects in the world. The country from Pegwell to Canterbury,—the bay itself in its whole sweep; point of Dover; the shipping in the Downs; the coast of France from Dunkirk to Boulogne; and in the afternoon when the cliffs are illuminated by the setting sun, every indentation of the coast is clearly discernible to the naked eye, appearing but a few miles distant; immediately under the windows, the shipping in Ramsgate harbour, and its celebrated pier stretching into the bay. In the evening the glimmering of the several light-houses in the distance, the lamps of the vessels passing through the channel, the bright light on the pier-head illuminating the foreground, form altogether one of the most enchanting scenes we have ever witnessed.

The baths, which are formed of white marble, are each placed in a room, lighted and ventilated from the ceiling, and communicating with their separate dressing-rooms. They are in length, width and depth, of the dimensions of the celebrated warm baths at Naples, which are so large as to allow invalids using the friction brush; to do so conveniently, without exposing any part of the body above the surface of the water. Pipes from the reservoirs and boilers, conduct hot and cold water into each, and another communicating with the drains allows it to run off after being used in a bath. The unsightly apparatus of pipes and cocks are here kept entirely out of view, and the whole apartment has quite the air of an ancient Roman bath. The dressing-rooms attached to each bath are of a very ample size, having a sea prospect, and they are fitted up with every thing that can administer to the comfort and pleasure of the bather.

The *shower baths* are constructed to have a supply either of *cold* or *hot* water, and the dressing-rooms which are attached to them, are furnished in the same manner as the others.

Vapour baths are also included in these establishments, and we understand modelled after those recommended and invented by the honourable Mr. Cochrane. From the several contrivances for increasing or decreasng the temperature of the vapour, it can be applied generally to the whole body, or topically to any particular part of it, with the greatest certainty and precision. *Medicated vapour* can be generated and applied with the same apparatus, and heated air can be introduced into this apartment to act as a Russian sweating bath, with the opportunity of using at the same time the *cold* or the *hot* shower-baths.

This establishment, however, has another claim towards being unique in kind. *Steam* has been applied to heating buildings for the purposes of manufactures, but this is the first successful attempt at employing it in a manner combining the greatest elegance with the greatest convenience and certainty. The steam is generated in a boiler on the outside of the building, from which it is conducted under the floors by pipes into an elegant vase placed in each dressing-room, standing in a niche made in the situation commonly occupied by the fire-place. In some of the coldest days in February, the temperature of the whole building was as high as 65°, and had all the delightful warmth of a day in summer. From the various contrivances for admitting the steam, or stopping it off from any particular part of the building, and directing it to any other particular apartment or series of apartments, the temperature can be raised at pleasure. In an establishment of this kind this mode of heating is a great improvement; the time required to trim the fires, and in keeping the apartments free from dust, if that is possible in an open fire-place, is saved to the attendants: and the invalids experience none of that unpleasant feeling occasioned by the opening and shutting of doors when dressing or undressing; and what has been so loudly complained of, the *unequal temperature* of the apartment and the *currents* of cold air from the chimney acting on the surface of the body, and in the cases of rheumatism, more particularly, defeating the object for which the warm-bath was prescribed.

When walking over the building, we had an opportunity of examining a model of a chemical apparatus for producing *chalybeate water*, by Galvanic action; but as we were informed, considerable improvements were making on the larger apparatus, we will take an early opportunity of laying a description of it before our readers.

The last and not the least improvement in this establishment is the very moderate sum which is intended to be charged for each bath. Those who have been accustomed to frequent Brighton for cheapness, will now here be agreeably surprised at finding infinitely superior accommodation for the *same money*.

LXXXVI. *On Indigogene.* *By* J. Murray, *Esq.*

To Mr. Tilloch.

Sir, — I beg to submit some remarks on the substance called by Brugnatelli *indigogene.*

It is obtained from indigo, a substance one of the products of the *Indigofera* and *Isatis*, and the varied species of which are natives of the East Indies, Cape of Good Hope, and New Holland. The extract from the letter of M. Van Mons of Brussels, in the Philosophical Magazine, first called my attention towards this curious body.

If indigo in the form of powder be projected on a red hot iron, a vapour of a blueish colour arises, and a red shade passes over the surface of the iron. This vapour exhales a peculiar odour somewhat resembling burnt malt; and when condensed, the indigogene is evolved under the form of fine needlelike crystals of a copper colour and lustre: viewed through a lens, sometimes arborescent groups appear. The mode which I adopt for procuring the indigogene is as follows:

A small glass capsule (a watch glass will suit) containing the powdered indigo is set in one of the sliding rings of the stand usually accompanying an Argand's lamp. A flat piece of glass is placed on the capsule, and it is then submitted to the action of heat. Water is first given off, then succeeds a reddish vapour, which attaches to the flat piece of glass (from which it is difficultly removed by spirits of turpentine); and ultimately, after the heat has been considerably increased, the substance in question is seen to pervade the surface of the indigo (which becomes a crust). It may be detached by means of a delicate spatula. The odour at first is unpleasant, but it becomes more mild and somewhat like that of sweet wort; or it may be obtained by heating indigo on a metallic plate. I have seen similar crystals on the sides of the indigo vat, from whence it seemed to have sublimed.

I find that indigogene is readily soluble in sulphuric acid and also in nitric acid, communicating to each a green colour. It is not soluble in muriatic acid, nor in a solution of boiling caustic potassa, nor in ammonia.

It is soluble in olive oil, even when cold, and in naphtha and cajeput oil with the assistance of heat. Ammonia added to the solution in oil of olives does not materially evolve the substance, but from naphtha and cajeput oil it is disengaged unaltered.

Indigogene is soluble in hot alcohol and sulphuric ether. It is instantly dissolved by an alcoholic solution of camphor being increased by heat, but is not so soluble in soap dissolved by spirit, communicating to these a blue tinge.

Indigogene when projected on melted nitrate of potassa detonates, and slightly explodes when mixed with oxymuriate potassa on the contact of sulphuric acid.

Indigogene does not combine with mercury by simple triture, but forms an amalgam aided by heat. Before the blowpipe when in a platina spoon it scintillates and inflames, a violet vapour evolves, and a blue shade crowns the surface of platina.— By percussion with oxymuriate potassa it fulminates with flame. The effects of various reagents may form the subject of a future paper.

I am, &c.

Liverpool, June 8, 1816. J. MURRAY.

LXXXVII. *On the State of the Manufacture of Sugar in France.*

By M. le Comte CHAPTAL.

[Concluded from p. 339.]

THE boiler into which the cleared juice falls should be about eight feet long, five and a half wide, and twenty-two inches deep. As soon as the bottom of this boiler is covered with the liquid, the fire is lighted, and the heat raised to ebullition as quick as possible. The instant the liquor begins to boil, sulphuric acid, diluted with twenty parts of water, is poured into it, in the proportion of a tenth part of the lime employed; the whole must be well stirred, that it may be completely mixed. In order to ascertain that there is no excess of lime or of acid in the liquor, it may be tried upon paper coloured with turnsole or curcuma. It is best to suffer the excess of lime to remain, and to employ no more of the acid, the moment that it gives to the curcuma paper a tint of a pale brick or deep white wine colour. After this operation, three per cent. of animal charcoal, well pounded to an impalpable powder, is mixed with the liquor, and immediately afterwards is added half of the charcoal that was used the evening before.

It has been observed that charcoal which comes from the preparation of prussian blue produces a better effect than that which proceeds from the distillation of animal matters in the manufactories of sal-ammoniac, which seems to retain the state of extreme division that is effected by the calcination; for it is ascertained that animal charcoal produces a greater effect in proportion as it is more attenuated and divided by pounding. M. Figuier of Montpellier was the first who discovered the superiority of animal charcoal to that of wood for decolorating liquids; and M. Derosne's application of it to the syrup of beet-root is so much the happier, as this charcoal, besides its property of decolorating, destroys the bad effects of the lime, and gives greater facility to the boiling.

After the last addition of charcoal the liquid is evaporated till it has acquired the consistence of from 18 to 20 degrees; it is then made to run into a smaller and deeper boiler, and is left at rest till the next day, when the boiling of the syrup is effected.

The Boiling and Refining.

The operation of boiling the syrup is the most delicate of any; but it has been rendered extremely easy by the improvements that have taken place in the preparatory operations, especially since the use of animal charcoal has been introduced. Many manufacturers have failed in boiling the syrup; and that which should be attributed to a bad manipulation, has generally been supposed to be owing sometimes to the non-existence of sugar in the beet, and sometimes to the almost insurmountable difficulty of extracting it. Now, this operation is become so easy that scum rises; it never burns during the boiling, and requires very little more care on the part of the workman who conducts it.

Previous to the boiling, the concentrated juice made the evening before, and which still retains some degree of heat, is filtered through a coarse piece of woollen cloth; it is then poured into a round boiler, two feet in diameter and eighteen inches deep, till it is one-third full, and is then heated to ebullition, which is kept up to the end of the operation. If it chance to burn, it is perceived by puffs of white smoke, which come from the bottom of the boiler, and burst through the surface of the liquid, spreading a pungent smell; the fire must be slackened, the liquor stirred, and the operation more carefully attended to. This accident was common three years ago, but by following the aforesaid process it is now become very rare. If the boiling mass swells, rises, and froths, it may be moderated by putting into it a small piece of butter, or by slackening the fire. The means of judging that the operation is going on well are, first, when it boils *dry* and with noise; secondly, when the syrup detaches itself from the skimmer without drawing into threads, and without adhesion; thirdly, when on striking the boiling mass with the back of the skimmer, the blow sounds dry, as if it struck upon silk; fourthly, when it produces very little skim; fifthly, when on taking up some of the froth or the bubbles out of the boil with the skimmer, the bubbles disappear directly and resolve into liquid: this latter character distinguishes the bubbles of the boil from those of the scum; lastly, we may be satisfied that the operation has proceeded well, if no traces of black can be perceived at the bottom of the boiler, and the surface appears clean.

The time proper to terminate the boiling of the syrup may be known by the following indications: first, by dipping the skimmer into the syrup, and on taking it out again pass the thumb rapidly over the edge, in order to take up a little of the syrup; work this drop of syrup between the fore finger and thumb, till it has acquired the temperature of the skin; then separate the finger and thumb rapidly: when it does not draw into a thread between the fingers the operation is far from being concluded; when it begins to form a thread the operation is far advanced, and then the experiment must be frequently repeated. The boiling must be discontinued the moment that the thread breaks *dry;* in this case the upper part of the broken thread shrinks towards the fore finger, forming a screw, and is never entirely lost in the mass that adheres to the finger. As soon as it is ascertained by this *test*, that the operation of boiling the syrup is completed, the fire is smothered, and a few minutes afterwards it is poured into the cooler, taking care to pour it high, that it may be mixed with air, for it is observed that this facilitates the crystallization.

Into the vessel called the *cooler* is poured the whole of the successive products obtained by the different boilings completed in one day.

In the evening, when the whole is thus collected in the cooler, the forms which are denominated *bastardes* are filled; the crystallization of the sugar immediately begins, and is almost always complete the next day; so that in 24 or 48 hours after it is put into the forms, these forms may, without inconvenience, be placed upon the pots for the melasses to run out.

A good crystallization is obtained when the surface is dry, the *paste* well grained and not syrupy, and when the surface of the base of the loaf of sugar cracks and is depressed towards the middle, which is known under the technical name of *fountain*.

I pass over several minor particulars of the process, which would be superfluous, as they are well known to all persons who are at all acquainted with the subject; observing, that in order that none of the juice may be lost, the scum, the residuum on the filters, and the settlings of the boilers, are all put into a lever press, to squeeze out what remains in them. It is very important that the juice be operated upon as soon as possible after it is extracted; for, if suffered to remain several hours, especially when unconcentrated, it undergoes alterations which injure the sugar, render its extraction more difficult, and considerably diminish the quantity.

I shall not dwell long on the process of refining, which is well known and understood; I shall only relate the improvements made in it by those who have been employed in extracting sugar from beet. M. de Rosne was the first who proposed to refine with alcohol, which is a very expeditious method, and the better adapted for the beet-root sugars, as it renders unnecessary a number of utensils which are requisite in the old method. To refine with alcohol, the operation must be commenced immediately as the melasses begins to run; for if any time is allowed for the sugar to dry, the melasses which moistens the crystals thickens, and forms a very hard coat upon the surface of the sugar, which the alcohol detaches with great difficulty: accordingly, the moment that the melasses begins to run, the surface of the sugar-loaf contained in the *form* is to be scraped, and a litre of alcohol at 36 degrees of commerce poured by degrees over the whole surface, the little orifice of the *form* being stopped; the base of the *form* is then carefully covered to prevent the evaporation of the alcohol. In two hours the orifice of the form is opened, and the alcohol runs into the pot, charged with a great proportion of the colouring principle; the operation may be repeated with half the quantity of fresh alcohol, and the sugar is then equal in whiteness to the clayed or fine powder sugar. The sugar is then melted and put into the boiler with bullock's blood. The operation is terminated by either claying or alcoholising it again; but it has been observed, that the last-mentioned process gives the sugar a more heavy look than the other, and renders it a little more friable; for this reason I use alcohol for the first operation, and claying for the second. The alcoholised loaves retain the smell of it for some time, which, however, goes off by placing them for a short time in a stove, or even on simple exposure to the open air. It is necessary to employ alcohol concentrated to 36 degrees; when it is weaker it dissolves a portion of sugar. It is not entirely lost, for by distillation it may be freed from the melasses, and employed again as before. Another method of refining has been proposed, which does not appear to me to possess the advantages of the new one just described, or even of the old one: it consists in dissolving 100 parts of raw sugar, and treating it with ten per cent. of charcoal and ten whites of eggs. When the loaf is in the form, they cause one and a half per cent. of white syrup to run through.

Account of the Expenses and Product of a Manufactory of Sugar from Beet.

The process I have described appears to me to be the most certain, œconomical, and simple of any that have come within my knowledge: and if the price of the sugar produced from it is greater than the sugar of commerce brought from the new world, it is still a new fact in science, and an object of curiosity to society. We shall now give an exact statement of expenses and receipts, in order that every one may be able to judge of the importance of this new branch of industry.

The expenses are comprised in the price of the beet, the manual labour for the extraction of the sugar, the interest of the sums spent in forming the establishment, the maintenance of the machines, the purchase of fuel, animal charcoal, and other less considerable articles. Beet-root is usually sold at ten francs per thousand weight; at this price the cultivator has hitherto met with a reasonable profit, especially when it is raised in suitable soil. By supposing it to be raised in land of a middling quality, but yet good enough to produce corn, we may calculate that an acre of ground will produce a crop of beet at the following expense:

	Francs.
1. Rent of an acre	20
2. Two deep ploughings	24
3. Two weedings	20
4. Purchase of seed	3
5. Weeding and harrowing	22
6. Gathering and carriage	40
7. Manure	50
8. Taxes	5
Total	184

We have in this statement charged the whole expense to the beet-root, although we have before said that the land appropriated to it was sown with corn towards the 15th of October, after the beet was gathered; and we may therefore charge to the corn the expense of the two ploughings, of the rent, of the taxes, and the manure; it is consequently evident that the amount may be reduced one-third.

We generally estimate the mean product of an acre of beet at 20 thousand weight, which fixes the price for the cultivator at 9 fr. 20 c.; but as the cleansing diminishes the beet as much as one-tenth, the 20 thousand weight are reduced to 18, when they come to be operated upon: we shall therefore reckon the price at 10 fr. per thousand to the manufacturer, always supposing that he grows his own beet.

To ascertain the other expenses, we will suppose that 10 thousand weight of beet-roots are operated upon every day.

	Francs.
1. Ten thousand weight of beet-roots	100
2. Two horses and a man .. .·. ..	9
3. Five women to the graters	3
4. Four men to the presses	6
5. Two men to the boilers	3
6. Animal charcoal	10
7. Acid, lime, and bullock's blood ..	2
8. Loss of alcohol used in the refining	4
9. Fuel	12
Total	149

As we suppose the manufactory to be at work only four months in the year, it is proper to assess upon these four months expenses of another nature, such as the interest of the money, the repair of the utensils, the salary of the superintending refiner, &c. Thus supposing the establishment to cost 30,000 fr. which is the *maximum* for a consumption of 10 thousand weight of beet per day:

	Francs.
The interest of the money for 120 days amounts to	16
The repair of the utensils and building	10
Salary to the refiner and his man	20
Petty expenses	5
Charges as above	149
Total	200

The expense of each day, therefore, employed in the consumption of 10 thousand weight of beet-root is 200 francs.

The produce of this 10 thousand is composed of three distinct parts—

parts—the sugar, the residuum or *marc* of the beet, and the melasses. In general the beet furnishes from three to four per cent. of raw sugar, and sometimes even from four to five. The quantity varies according to the state of the weather and the expertness of those who work in the establishment. Supposing only three per cent. is extracted, the 10 thousand weight of beet will then produce 300 lbs. of raw sugar, which, reckoning the daily expense of 200 francs, brings the price of the raw sugar to about 13 sous, or 65 centimes per pound. Besides the produce of the sugar, there is another which deserves consideration; this is the cuttings and the residuum of the beet after the juice is expressed from it. The cuttings, as we have before observed, compose nearly a tenth part of the weight of the beet; they consist of the tops, the radicles, and the earth that adheres to them. On a thousand weight of cuttings, off 10 thousand of beet, there is at least a good half which is excellent food for pigs, who are very fond of it. The residuum or *marc* is a still more important article: supposing 70 per cent. of juice to be extracted, the 10 thousand weight daily consumed furnishes 1500 kilogrammes, or about 30 quintals, of *marc*, which is a very valuable food for horned cattle. This food, which is nearly dry, has none of the inconveniences of herbs or aqueous roots, or of dry fodder; it produces no putrefaction like the first, and does not heat them, or occasion obstructions, like the latter; it contains almost all the nutritive principles of the beet, being deprived of only about 60 per cent. of water, three of sugar, and a little extract and gelatine; oxen, cows, and poultry are very fond of it, and it fattens them better than any other food; sheep and milch cows that are fed with it give much more milk, and of an excellent quality. Thus an establishment of the magnitude of the one I am speaking of, may fatten annually from 50 to 60 oxen, or from four to five hundred sheep, with the residuum only.

The melasses is a third product not to be overlooked; a thousand weight of beet will produce nearly 240 pounds, which may be sold at the rate of ten or fifteen francs the quintal, or the fifty kilogrammes; or it may be retained to be fermented and distilled in order to extract the alcohol. When the melasses is kept for distillation, it is diluted with water till the liquor marks from seven to nine degrees; it is then carefully mixed with yeast, or the leaven of barley paste, tempered with warm water, in the proportion of two pounds of the first-mentioned to ten quintals of liquid, and six pounds of the last-mentioned. The vessels that contain the fermenting liquor should be placed in a stove, where the heat is constantly from sixteen to eighteen degrees of the centigrade thermometer; the fermentation soon appears, and is terminated in a few days. The distillation [illegible] be effected in the improved alembic of *Adam* and *Bernard*, then the alcohol has no bad taste, and it can be obtained to any desired degree by a single distillation. This alcohol has the peculiarity of being infinitely more pungent than any other at the same degree of concentration. One hundred litres of melasses give nearly 33 litres of alcohol at 22 degrees. Before the residuum is given to the cattle it may be fermented by diluting it with a sufficient quantity of water, and distilling it afterwards; by this means about four per cent. of alcohol may be extracted from it; but this operation requires a degree of manipulation that induced me to abandon it; nevertheless, it gave rise to an observation that may be useful to be known to persons engaged in the same object. I had conceived the idea of passing water over the residuum, and using it afterwards to temper the melasses; this lixiviated water marked from two to four degrees; I proceeded afterwards to the fermentation in the usual manner; the fermentation proceeded with facility; when it had terminated I submitted the liquor to distillation, and was surprised to find that it yielded less alcohol, and that towards the end of the operation the liquor expanded or swelled, and passed from the boiler into the worm. I was soon convinced that the melasses had not participated in the fermentation, that it had remained undisturbed, and that only the lixivium of the residuum had fermented. This experiment, several times repeated, afforded always the same results: it appears that the melasses mixes without combining with the water of the lixivium, which ferments first, and stops the motion of the melasses. The ashes of the *marc* or dregs will furnish nearly one per cent. of potash.

General Remarks.

We see by the above details, that France is able to manufacture, at a low price, as much sugar as is necessary for home consumption. But there are still two or three questions necessary to be examined, in order that nothing may be left undone in a matter of so much importance.

1st. Whether the sugar made from beet-root is of the same nature with that of the cane?

2d. What advantages may be derived to agriculture from establishments for the extraction of sugar from beet-root?

3d. Whether it is the interest of France to manufacture this sugar?

4th. From what reason have most of the establishments erected for this purpose been given up?

In regard to the first question; *whether the sugar made from beet is of the same nature with that of the cane,* I shall observe, that we are at this time acquainted with three distinct kinds of sugar, all susceptible of yielding alcohol by fermentation, but each differing in particular properties: one is uniformly in a liquid state, another is always a powder incapable of crystallization, and the third is composed of very regular crystals. The first kind, or liquid sugar, exists mostly in vegetables and fruit; it constitutes the syrup when the juice is properly concentrated by evaporation.

The second sort is firm and dry, but without being susceptible of crystallization: the sugar of the grape is of this species, as well as that of honey, and that which is produced from the adulteration of starch by sulphuric acid.

The third kind is susceptible of crystallization; and the crystals take the form of a tetraedral prism, terminated by a diedral summit. This species is found in the sugar-cane, the beet-root, in the sugar-maple, the chesnut, &c. This sort is the most esteemed and the most sought for, because it has the pleasantest taste, is sweetest in proportion to the weight, is the most easily used, and is the most agreeable to the sight.

There is now not the least doubt among enlightened men as to the perfect identity of the different sugars that constitute the third sort; and when the sugars are brought, by the process of refining, to the same degree of whiteness and purity, the most experienced can detect no difference in them.

Undoubtedly, when in the first attempts of the manufacture the sugar produced from beet-root was frequently sent into the market burnt, ill prepared, and badly refined, the consumer had reason to reject it, and found it very different from the sugar of Hamburgh or Orleans; but even then the well-informed ranked it in the same species, and attributed the difference to the imperfections of an infant process, rather than to the nature of the principles. Long since our celebrated colleague, M. Haüy, had proved that the form of the crystals was the same; several establishments had exhibited results analogous to those of the colonies; and it was natural to conclude that the same perfection would be gradually attained by all. We know that at all times woollen cloths have been manufactured of the same materials, and that yet the cloths of the tenth century were by no means comparable with those of the eighteenth:—we know that every art has its period of infancy, but that in the present age this infancy is of shorter duration, by reason of the progress of knowledge.

That which was predicted has taken place; and in less than two years the manufacture is wonderfully improved; it is simplified to that degree that it is now confided to the workmen, and there are few operations that afford more certain and uniform results: thus the product of the beet-root establishments is circulated in commerce without opposition, and the consumer gives the same price for it as for that of the cane of the same quality. It has been said that this sugar is lighter than the cane sugar, and consequently that the same quantity in bulk does not sweeten so much: trifling as this accusation is, I cannot admit it; I employ the same forms as those used at Orleans, and each furnishes a loaf exactly of the same weight as in the refineries of Orleans.

For three years I have used at my own table no other sugar than that of my own manufactory; and my friends, who had no suspicion of it, have seldom failed to compliment me on the beauty and good quality of this sugar.

I have already observed, that the sugar refined by alcohol exhales, for some time, a disagreeable smell; so that if it is sent to market immediately the consumer will reasonably complain of it; but this is no defect in the sugar, it is the fault of the proprietor, who should not offer it for sale before the smell of the alcohol is gone off. Thus I have shown that the sugar of beet-root and that of the cane are strictly of the same nature, and that no difference can be found in them.

As to the second question, *whether agriculture can derive any advantage from the establishments for extracting sugar from beet*, I reply, that agriculture must unavoidably derive great advantage from them, for whatever tends to vary the crops, and augment their number, is beneficial to it: in this view therefore the culture of the beet-root is advantageous; for besides affording an intermediate crop, it doubles the product of the funds, and does not cause the loss of a single grain of corn. The cultivation of the beet-root also renders the soil more light, and clears it of weeds. The manufacture of the sugar is not less useful than the cultivation of the plant. First, the residuum or *marc* of the beet affords food for the horned cattle and pigs of a large farm for four winter months, November, December, January and February. Supposing France contains two hundred manufactories, each working upon ten thousand weight of beet-root daily, the *marc* or residue from them will fatten from ten to twelve thousand oxen, and from two to three thousand pigs. Secondly, these manufactories have the advantage of employing the horses and men of a farm during the dead season, and of giving work to others, who during these four months would otherwise be condemned to idleness. Independent of the men employed in the cultivation of the plant, the cutting and the extraction of the sugar may employ from five to six thousand persons during the winter, supposing two hundred establishments to be at work.

To the third question, *whether it is the interest of France to multiply these manufactories*, I may answer, that France can have no other interest than that of her inhabitants; consequently, whatever augments the mass of labour, or multiplies the productions of the earth and of industry, and enriches the agriculturist, must merit protection on the part of the government. In this place, the great consideration of the colonies presents itself, and I do not pretend to resolve a question of such high importance; I shall confine myself on this subject to the statement of a few remarks, which I submit to the wisdom of government, and to men more competent to decide than myself. I shall not say, with some writers, that the colonial system does not interest the nation, under the pretext that the colonies bring nothing into the public treasury, that they require the support of a very expensive marine, &c. I know that the colonies open a market for the products of our industry and of our soil, that they supply our manufactories with raw materials, and that they give great activity to commerce. Under all these relations, the colonies have hitherto been one of the principal sources of public prosperity; but if all these advantages can be obtained in the bosom of France itself, if the indigenous fabrication of sugar and indigo can replace the sugar and indigo of the new world, at the same price and of the same quality; if this new branch of industry augments the mass of labour among ourselves, and enriches our agriculture, without depriving us of any of its products; it is evident that there remain against the colonies, without compensation for any superior interests, the annual expense which they occasion, and the numberless chances of war, which all at once sacrifice our fortunes, and force us to privations, when a formidable marine is unable to obtain dominion, or at least equality, upon the seas. These reasons might be strengthened by looking at the actual state of the colonies: but God forbid that I should pretend to turn the attention of government from an interest equally great to the metropolis, and from its paternal solicitude for the unhappy colonists who have been despoiled of their property! I only desire, that at the present moment the establishments of indigenous sugar may be encouraged, so that, their products being upon an equal footing with those of the colonies, we may share with foreigners the commercial relations which are limited to the exchange of our colonial commodities, especially sugar, against the productions of their soil. This becomes the more important, as our principal trade with Hamburgh and the northern countries consists of colonial commodities, for which we receive wood for building, metal, potash, hemp, flax, and tallow; and that when these great means of exchange happen to fail, England has the advantage of this immense commerce.

The fourth and last question is, *from what cause have the greatest part of the establishments failed that were formed for this purpose?*

Persons who form a superficial judgement of the arts, are persuaded that the manufactories of sugar from beet cannot support a competition with those of the cane, and they are now confirmed in their opinion by the failure of so many of the establishments that were formed before the peace. To this we might reply, that it is sufficient that some of them still remain, notwithstanding the importation of foreign sugar, to prove that our manufactories are capable of rivalling them; but I prefer in this place to point out the causes of the failure, and to establish certain principles which may serve to guide those persons who may in future undertake to form fresh establishments.

When the extraction of sugar from beet-root was at first desired, the government excited the zeal of all France by the encouragement it offered: every where the beet was sown, and numerous establishments were formed, without any previous consideration on the nature of the soil, the expense of the cultivation, or the saccharine quality of the root. Vast buildings were erected at a great expense, graters and presses were bought, the effects of which were not understood; and frequently the whole made ready to set to work, without the least mistrust of the process intended to be followed; and sometimes even unprovided with a man capable of conducting the operations.

The rational progress of a new branch of industry was not attended to, great losses were suffered, and they might have been expected. In some places the beet was found to contain no sugar when it was operated upon; this was the cause of the fall of all the establishments in the south; in others, defective processes were employed, and only syrup could be extracted; and the cultivation or purchase of the beet has cost so much, that the product has not balanced the expense. This inconsiderate mode of proceeding was necessarily the ruin of most of the undertakings; and as every one is apt to reason from his own experience, whether good or bad, a general prejudice soon prevailed against the success of this manufacture. On the other hand, the bad quality of the sugar which some manufacturers sent to market, has contributed, in no small degree, to disgust the consumer.

It would doubtless have been better to have sought out the causes of failure, and to have studied the methods of the prosperous establishments: but public opinion is not always so just, it often adopts a novelty without examination, and still oftener proscribes it without reason. Neverthelesss the repeated attempts in every part of France have afforded results by which some have profited, and have at length furnished us with a positive knowledge of the culture of the beet-root, of its product, and with a certain, easy, and œconomical method of extracting the sugar from it. Experience has also taught us, that the manufactories of sugar from beet-root can only prosper in the hands of proprietors who cultivate the plant themselves, and consume the residue upon their own demesne: indeed, it is only necessary to take a view of the advantages which this manufacture affords, when connected with a large farm, to be convinced of the great difference in the two cases.

First. The proprietor who cultivates the beet, obtains it at a lower price than he who buys it: this difference is immense; especially if we consider, that as it is an intermediate crop, the expense of ploughing and manuring may be laid upon the crop of corn that succeeds it.

Secondly. The residue of the beet will feed nearly all the horned cattle of a large demesne, during the four severest months of the year; whereas if sold it does not produce half the benefit.

Thirdly. The carriage, and most of the operations, may be executed by the horses and men belonging to the farm; while, in the other case, all these must be paid for this purpose solely, and for a limited time, which increases the disadvantage.

Fourthly. Manual labour is dearer in towns than in the country.

Fifthly. Fuel is always dearer in towns than in the country, especially wood, which may be employed in some of the operations.

This new branch of industry, therefore, must be established on extensive property, for on such only it can succeed. Independent of the advantages of situation, we may add, that the erections necessarily depending on a large farm will mostly suffice, without any further expense, for the purposes of the new manufacture. I could mention two establishments of this nature, which required no more additional buildings than amounted to the expense of 300 francs. These establishments are in a prosperous state at this moment, and are about to commence their fifth year.

The large proprietor, accustomed hitherto to easy harvests, will probably feel unwilling to undertake this new system, because he may imagine himself not sufficiently acquainted with it: but if he considers that all the expense of experiments has been incurred by others; that the processes we have described are easy and certain; that the calculations, being deduced from experience, are accurate; that the distilleries of grain and potatoes, which are formed in almost all the northern farms, require information equally extensive, without affording so much advantage; since, besides the food for cattle, and the product of the alcohol, which is more abundant from beet-root than from grain, we have also the production of the sugar; he will see, that he may at the same time improve his land, and assist to enrich the country with a product which has become one of the first necessity.

LXXXVIII. *On Aërial Navigation.* *By* JOHN EVANS, *Esq.*

To Mr. Tilloch.

SIR,—I AM much gratified at finding that my paper inserted in your Magazine for November last has given rise to some able communications in your subsequent numbers. My engagements at the College of Edinburgh during the past winter will, I trust, prove a sufficient apology for my apparent neglect in not noticing them at an earlier period.

The claim of priority of invention urged by Mr. Edgeworth I am perfectly willing to admit, but must at the same time entirely disclaim all knowledge of any experiments in which the inclined plane was proposed as the means of directing balloons. Sir George Cayley's papers in Nicholson's Journal were equally unknown to me.

Sir George's paper in your number for February commences with mentioning a difficulty attending the method by which I had proposed to apply the action of the inclined plane. The plane being merely *suspended* below the balloon, would during the descent be urged by its own weight only, and consequently would not receive an adequate resistance from the air. This difficulty, however, may be easily obviated by *fixing* the plane *close* beneath the gallery (taking due precautions as to safety from fire), and thus making the weights of the balloon and plane act in conjunction. The sole reason of my placing the plane so far below the balloon was to allow the air to reoccupy the partial vacuum which might be formed beneath the latter whilst ascending, and tend to render the effect of the inclined plane less complete. But if the plane be firmly attached to the inferior part of the balloon as now proposed, a very slight elongation of the diagonal yards will allow the expansion of an additional quantity of canvass sufficient to compensate the loss of resistance occasioned by the proximity of the balloon.

Sir G. afterwards gives another mode of applying the same principle, which, it is rather remarkable, is almost the same as I tried last summer, and to which I alluded in the last paragraph of my former paper. I do not mention this circumstance with the view of questioning the originality of Sir George's idea, but merely because it may be useful, in the prosecution of further experiments, to know what constructions have been already examined, and what difficulties have impeded their execution.

It appears a very obvious improvement to make the balloon itself furnish the required oblique surface as well as the moving power. In proceeding to submit this to experiment, the first question that offered itself was, what should be the shape of the balloon in order to obtain the oblique surface most conveniently? From previous experiments, I was well acquainted with the difficulty of distending the balloon by framework however lightly made. I therefore sought some shape which should not require such assistance, but be such that the balloon could not assume any other figure without diminishing its capacity. The shape of the first balloon constructed with this view, was that of two flattened hexagonal pyramids united by their bases. The balloon when inflated did not perfectly retain the above figure, but became rounded at all the corners and bounding lines. The aperture for the fire was not at the apex of the inferior pyramid, but on one side of it, and a weight was attached which might be adjusted so as to keep the balloon at the requisite obliquity. The balloon was elevated in the room where I made the experiments detailed in my former paper, but no sensible effect was observed. Being afterwards launched in the open air with a small side sail attached, it assumed the same rotatory motion as a balloon furnished with an inclined plane.

In this experiment much inconvenience arose from the constant tendency of the hot air to ascend to the higher extremity of the balloon, which was consequently always endeavouring to attain a perpendicular position. It is much to be feared that this circumstance would be a material obstacle to the success of balloons constructed after this plan on a large scale; which consideration inclined me to prefer the original method of suspending the plane at some distance below a spherical balloon. The experiments on the comparative advantages of the two constructions are however at present too few and imperfect to warrant a decision as to the superiority of either of them.

Sir George in his last paper gives a description of a hydrogen gas balloon furnished with wings or oars, which are to be moved alternately up and down by a steam-engine. It appears to me that by a force being communicated to these wings in one direction, which force is almost immediately destroyed in order to give an impulse in the contrary direction, a considerable loss of power must be occasioned. I would therefore beg leave to suggest that a large wheel with oblique vanes (like the fly of a smoke-jack)

jack) be substituted, which by revolving continually in one direction would attain the desired object, with no more waste of power than what would arise from the additional machinery necessary to obtain a rotatory motion from the steam-engine.

Well knowing that the further advancement of this interesting subject must depend on the *united* exertions of the friends to science, I shall with pleasure enroll my name in the list of subscribers for the completion of so desirable an object.

I am, sir,
Yours respectfully,

Pullin's Row, Islington,
June 11, 1816.

JOHN EVANS Jun.

LXXXIX. *On the Cosmogony of Moses. By Dr.* PRICHARD; *in Reply to* F. E——s.

To Mr. Tilloch.

SIR, — I AM sorry to find myself under the necessity of trespassing upon your patience, by once more directing your attention to the Cosmogony of Moses.

I shall make but a few remarks on the last letter of your correspondent F. E——s. It contains nothing new except a *tirade* about *torches* and a *chateau en Espagne*, from which I can understand nothing except that the writer intends to be facetious. I wish that his method of treating the points in controversy were equally novel and ingenious, and that he had not contented himself with repeating the same objections to which I have before, as I trust, sufficiently replied. He is still determined to find contradictions between propositions which have no relation to each other, and quarrels even with the words in which they are expressed. I shall not stay to notice mere cavils, but shall content myself with a specimen of the mode of reasoning adopted by this pertinacious critic.

In my last paper I hinted at the instance of St. Matthew and St. Luke, in order to prove that inspired writers have chosen to avail themselves of historical documents when such sources of information were to be found. Mr. F. E. seems to allow the force of this example, but denies that it leads to any inference with respect to Moses; and the exception he takes against it is to the following purport. St. Matthew and St. Luke found preexisting documents, which it only required in them human sagacity to adopt; but Moses, it seems, had nothing but the light of revelation to guide him: consequently he made no use of records. Now there is one grand objection to this conclusion;

viz. that it takes for granted the chief thing intended to be proved.

I here beg to disclaim the merit kindly ascribed to me by F. E——s, of having formed an "*ingenious imagination*" respecting *sorts* of inspiration, and to enter a protest at the same time against the pretensions of those who, like that writer, talk largely of "circuitous inspirations" and "immediate inspirations." Such knowing persons indeed would be very great prizes in these iron times, if we could be sure that their information is genuine; but until they point out the sources whence it was derived, we must be excused from paying deference to their superior attainments. I am such a tyro in these matters, that I cannot even see any force or shadow of meaning in the objection advanced by F. E——s against my former positions, and founded by him on the supposed *sort* of inspiration which is to be ascribed to Moses. Far be it from me to pronounce the particular time, place, and manner in which the most ancient revelations were made, or "*to ascertain the person favoured with them*;" nothing less than which, it seems, will satisfy my unrelenting inquisitor. I have no one opinion or hypothesis on the subject, but leave all such sublime matters to F. E——s and the *winged folk* of Aristophanes:

> . . . . τοῖς αἰθερίοις,
> τοῖσιν ἀγήρως, τοῖς ἄφθιτα μηδομένοισιν,

while I take up my humble place among the ἀπτῆνες ἐφημέριοι, the *unfledged mortals* who are doomed to grope upon the surface of the earth, and see but a short space before them. My reason for believing that Moses was not the original author of the Cosmogony, is not any speculative opinion concerning subjects beyond my comprehension, but the fact that the same record is found among distant nations, whose history has been totally unconnected, from a period long antecedent to the age of Moses; and I presume that one fair inference from historical facts will weigh down a hundred hypotheses concerning *sorts* of inspiration.

But although I do not pretend to be so sagacious as F. E——s in occult matters, I am far from intending to depreciate the character of Moses. His writings display a mind so free from those prejudices which enslaved the greatest philosophers in the most enlightened ages of antiquity, that the extent of his wisdom appears quite out of the course of nature. His freedom from superstition is the more astonishing, if it be true, as the Egyptian historians assert, that Moses was originally a priest of Heliopolis.

I have not, as Mr. F. E——s would insinuate, rested my interpretation of the word *day* on the authority of Josephus and

Philo. Yet as it has been my endeavour to show that every part of the first chapter of Genesis is more or less metaphorical, I do not perceive how the figurative sense imputed to the whole of it by those writers can be represented as wholly foreign to my purpose. At any rate it is quite "*obvious to ordinary understandings*," that the opinions of two such writers as Josephus and Philo should be taken into the account by a critic who pretends to estimate the notions entertained by the Hebrew people, and to rest on them the chief stress of his argument.

I shall not enter further into the inquiry what place corals and bivalves hold in the scale of creation, whether they are, as F. E——s declares, *locomotive animals*, or approach to the character of vegetables. The question has been decided in my favour by a third person unprejudiced in behalf of either party, who has shown himself to be perfectly well informed respecting the points in controversy. In fact, it is impossible that any thing can be said more clear and satisfactory than the remarks of Mr. Horn in the 341st page of your last number.

I must now advert to the strictures of Mr. Horn upon some parts of my last paper; and I shall be brief, as I do not feel myself particularly interested in the question which they involve. Before Mr. Horn passes a peremptory sentence upon the opinions of Michaelis, I wish he would take the trouble to consider and refute the arguments of that profoundly learned writer, and to furnish some other explanation of the facts from which his conclusions seem to follow as fair inferences. I confess that I have been accustomed to consider the opinions of Michaelis as indisputable; but if Mr. Horn can enable me to think otherwise, I shall become a willing convert to his doctrine. When Mr. H. has vanquished the German professor, he may find some amusement in disposing of our obstinate countryman Dr. Middleton, who has displayed profound erudition and a most vigorous intellect in defence of similar opinions. For the present, I hope to be excused if I understand the texts which Mr. H. has cited, in a more limited sense than that which he has affixed to them. I beg however to assure him, that I have no design to insinuate that Moses borrowed his account of the creation from the Egyptians. I doubt not that the primitive traditions were preserved in greater purity by the Hebrews than by any other nation of antiquity;—neither do I intend to impute to the whole Hebrew nation so superstitious a notion as anthropomorphism. The vulgar were anthropomorphites, as probably are many ignorant persons in every country: but it cannot be doubted that well-informed and philosophical men interpreted the passages pointed out by Mr. Horn in a figurative manner; and it was for the sake of drawing this distinction, and availing myself of the example afforded by it, that I first hinted at the anthropomorphism of the Hebrews.

I shall conclude as briefly as possible. I have shown that if we receive the *days* of the Hebrew Genesis in an extended sense, a most important series of coincidences is developed between the epochs of nature and the events recorded by Moses. I have proved that the word is capable of the sense I affix to it without violating the common forms of speech. It is indeed allowed by F. E——s that such a figurative sense is applied to it in a variety of languages. If we only grant Moses the same indulgence which any common writer would have a right to expect at our hands, we are bound by every rule of candid criticism to adopt the meaning most favourable to his accuracy. The question might therefore be decided on highly probable grounds, if no further information could be obtained, and the Mosaic Cosmogony might be considered as a philosophical and rational narrative of events, rather than a mythological rhapsody. But further, I have shown that two very ancient nations, viz. the Hindoos and the Etruscans, have preserved this same document of the Cosmogony with some variations, but with this remarkable difference, that where Moses mentions "*days*," they expressly define long periods of time or ages. This is precisely that very confirmation that was wanting, in order to convert a highly probable conclusion into a legitimate inference.

If your correspondent has any new observations to offer on this subject, I shall feel it incumbent on me to reply to them: but if he still confines himself to the "*crambe repetita*," I shall not exhaust the patience of your readers by agitating a frivolous dispute, but shall be contented with applying to myself the remark of a celebrated French writer, that "a man may have the right side of an argument, though he should not have the last word in the controversy."

I remain, sir,
Your very obedient servant,
Bristol, June 14, 1816. J. C. Prichard.

XC. *On Meteorology, &c. in reference to Mr.* Forster's "*Researches about Atmospheric Phænomena.*" *By the Rev.* T. Drummond, *of Norwich.*

To Mr. Tilloch.

Sir,—The proposition to publish a Meteorological Journal induces me to offer a few remarks on the subject.

As no precise outline of the plan has been brought forward, it may be inferred that the new work is intended to collect testimonies from a variety of places in this empire, and in foreign countries, relative to the barometer, thermometer, wind, and weather; at particular periods. If it be also in contemplation to specify agreeably to the excellent classification of Luke Howard, Esq. the various clouds preceding or coincident with the changes of the weather, I shall not dispute the utility of such records; but since the effects produced in our glasses, the fluctuations of the wind, the varying figures of the clouds, and the changes of the weather, are subsequent to some prior causes in the operations of Nature, meteorological records *so limited* will serve only to register what has occurred, without affording any presage of what may occur.

If the philosophers will enlarge the table, and note at the same time the situations of the planets, they will probably find in sidereal operations on the atmosphere not only causes prior to the above effects; but from the revolutions in the planetary system they may by calculation anticipate the probable variations in the atmosphere at subsequent periods.

Pliny speaks of those who could predict thunder-storms to a day. If we are not competent to speak with the same positiveness, it must be ascribed to the fashionable neglect of the subject. If philosophers will cease to disregard a branch of science almost exploded, they may in time acquire the accuracy of judgement to which Pliny alludes.

I consider it unquestionable, that at certain positions of the planets snow or rain never fails to occur.

From the want of a greater number of modern testimonies founded on observation, the incertitude at the period of other positions is great. There is, perhaps, less reason for confidence in an island than on a continent, and the variety of weather may be greater near the sea coast than in an inland county: but if the gentlemen think proper to prosecute their plan with such additional remarks, they may be expected to relumine the lamp of the ancients, and probably obtain for us some new light on the subject.

A respectable philosopher of the present day has given an opinion on this subject, and I beg leave to question that opinion, and to note an apparent inconsistency combined with it.

I presume the gentleman as a philosopher has no object but the development of truth. He will not find in me a disrespectful opponent; but, as the work to which I allude may be supposed by some readers to militate against the hypothesis I am attempting to maintain, he will excuse my quoting a passage in his publication "Researches about Atmospheric Phænomena."

"*The old notions of astrologers about the conjunctions of the planets involve too many palpable absurdities to allow us to* *collect any useful information from their writings. But it is* certain *the place of the moon has some influence on the weather. That changes of weather oftener take place about the full and* new moon, *and about the* quadratures, *than at other times, is* really a fact *founded on long observation.*"

I apprehend that the moon in conjunction with, or in opposition to, the sun, are two data whence the old notions of the astrologers were formed; and if the quadratures may be considered productive of any variations in the atmosphere, the moon's position with respect to the sun appears to be the only rational mode of accounting for them.

If Mr. Forster should be induced to consider the operations of Saturn, Jupiter, &c. possible, he will most probably be convinced that the variations in the atmosphere are not dependent solely on the moon.

I remain

Yours respectfully,

T. DRUMMOND.

Gray Friars Priory, Norwich,
June 14, 1816.

XCI. *Report of the National Vaccine Establishment, for the Year* 1815; *dated* 31*st May* 1816.

To the Right Honourable Lord Viscount SIDMOUTH, *Principal Secretary of State for the Home Department, &c. &c. &c.*

National Vaccine Establishment, Leicester-Square,
May 31, 1816.

MY LORD,—WITHIN the last year the surgeons of our different stations in London have vaccinated 6,581 persons, and have distributed to the public 32,821 charges of vaccine lymph. We cannot state precisely what the sixty-eight honorary and corresponding vaccinators may have effected in the country, as returns are not always sent: however, we have ascertained that those practitioners whom we have supplied with lymph have vaccinated 42,667 in the course of the year.

We have the satisfaction of informing your lordship, that we have furnished the means of disseminating this blessing in the island of St. Domingo; and that the director has received the annexed letter from the government of Hayti on that subject.

It is equally gratifying to us to state, that by the ingenuity of Mr. Giraud of Faversham, means have been devised of preserving the lymph in a fluid state; by which we have just reason to hope that it may be found efficient in any climate, and for any space of time.

Your lordship has probably been informed, that in consequence of the decisive measures adopted in Russia, Sweden, Germany, France and Italy, the small-pox has become a very rare disease in those countries; and that, by like means, it is no longer known in Ceylon and at the Cape of Good Hope. It is a source of sincere regret to us, that it should not be equally so in this kingdom; and still more so, as this is not attributable to the casual occurrence of that disease; but, we believe, entirely to the practice of inoculation, which seems to be adhered to on interested or mistaken motives.

In Edinburgh, Glasgow and Norwich, Inoculation is disused; and, in consequence, the small-pox is scarcely known. In the country about Aberystwith in Wales, and Bawtry in Yorkshire, it has entirely disappeared. The reverse is found unhappily to be the case in Portsmouth, Bristol and London. In the metropolis alone, the mortality by small-pox may be estimated at a thousand annually: perhaps throughout the United Kingdom it is not less than ten times that number.

We beg to conclude by stating, that it appears to us, this waste of human life can be prevented only by such legislative enactments as will entirely put a stop to inoculation for the small-pox.

The Board is happy in stating, that it has no occasion to ask Parliament this year for any sum of money beyond that usually granted.

(Signed) J. Latham,
(President of the Royal College of Physicians) President.
Henry Cline, Master of the Royal College of Surgeons.
Henry Halford, M.D.
William Lambe, M.D.
Joseph Agar, M.D.
J. Coxe, M.D.
} Censors of the Royal College of Physicians.
William Norris,
James Earle,
} Governors of the Royal College of Surgeons.

By order of the Board,
James Hervey, M.D. Registrar.

Palace of Sans Souci, Feb. 5, 1816,
13th Year of our Independence.

The King of Hayti to Mr. James Moore, Director of the British National Vaccine Establishment, &c. &c.

Sir,—Mr. Prince Sanders has presented me with the work which you sent me on the small-pox: I have accepted this work with pleasure, and thank you infinitely for your honourable and obliging attention, and the interest which you evince for the Haytians.

The precious discovery of vaccination is too important to human life, and does too much honour to humanity, not to induce me to adopt it in my kingdom. On the arrival of Mr. Prince Sanders, I put vaccination in use with a view to make it generally followed by the Haytian practitioners;—we have an innumerable quantity of children to vaccinate.

It is my intention to give every possible latitude to the happy results of this immortal discovery, which I had not hitherto been able to put in practice in consequence of the disappointment which I met with in the applications I made at Jamaica, St. Thomas, and in the United States of America, relative to this object, the salutary effects of which I am well acquainted with. This benefit will still add to the gratitude of the Haytians for the great and magnanimous British nation.

I have charged Mr. Prince Sanders to testify to you personally my sincere thanks.

(Signed) HENRI.

XCII. *Essay towards a natural Classification of simple Bodies.*

By M. AMPERE*.

WHEN the arbitrary hypotheses which had long led chemists astray were banished from science, and it was ascertained that we were to consider as simple, all the bodies which had not yet been decomposed, the number of these bodies was not two-thirds of what they are now: this number successively increased as the processes of chemical analysis were applied to compounds which had not yet been analysed, or which had been so but imperfectly. Every time that a new simple body was discovered, a further term of comparison was obtained, and new relations were observed: it became necessary sometimes to restrain, and sometimes to generalize, the first views of the fathers of modern chemistry; and the want of arranging simple bodies in an order which renders more sensible their mutual relations, and facilitates the study of their properties, became more and more felt. This order may be purely artificial, like the systematic classifications which were at first resorted to in the other branches of the natural sciences: it may also be deduced from the *ensemble* of the characters of the bodies which we propose to classify; and by constantly uniting those presented by the most numerous and essential analogies, they will be to chemistry what the natural methods are to botany and zoology.

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* *Annales de Chimie et de Physique*, tome i. p. 295. March 1816.

Hitherto chemists have confined themselves to ranging simple bodies according to the degree of their affinity for oxygen, and the nature of the combinations which they form with it. They ought naturally to have adopted this kind of classification, when they thought that the properties which characterized the oxygen belonged to it in a manner so exclusive that no other body could be associated with it. But nowadays that new facts, and a more accurate interpretation of the facts already known, have rectified whatever was too absolute in the theory established by the celebrated Lavoisier; and now that other substances have presented similar properties; it appears to me that we must of necessity banish from chemistry the artificial classifications, and begin by assigning to each simple body the place which it ought to occupy in the natural order, by comparing it successively with all the rest, and uniting it with those which resemble it by a greater number of common characters, and particularly by the importance of those characters. The first advantage which will result from the employment of such a method, will be to give us a more exact and more complete knowledge of all the properties of simple bodies; and frequently to refer to general laws a multitude of isolated facts. Another advantage will arise from this, namely, that after having ascertained among those which we shall have thus united, analogies so multiplied that we cannot refuse to regard them as connected very closely in the natural order, we shall be led to try upon some, experiments similar to those which have been attempted with success on others. A classification, which should have induced every person from the very origin of modern chemistry to consider all the salifiable bases as belonging to one and the same class of bodies, would have taught chemists to place potash and soda in contact with iron at a high temperature, and potash and soda would have been discovered twenty years sooner. When it was ascertained that chlore was a simple body, it was at first compared to oxygen; and it was only when M. Gay-Lussac remarked its analogies with sulphur, that he was led to a discovery, the consequences of which upon the ulterior progress of chemistry can only as yet be guessed at, viz. that of the chloric and iodic acids, and of the perfect analogy of the chlorates and iodates with the nitrates. A second approximation followed almost instantly by another discovery to which it naturally led, that of cyanogene, and of the true nature of the hydrocyanic acid. Finally, these very analogies doubtless guided M. Dulong in the work which he communicated to the Institute on the 7th of November 1815, in which we see that the oxalic acid is composed of carbonic acid gas and hydrogen gas combined in the ratio of two to one in volume; that this acid, which he calls in consequence hydro-carbonic acid, conformably to the established nomenclature, is united with the oxides in such a proportion, that the volume of hydrogen which it contains is double that of the oxygen of the oxide, so that when the latter is not very difficult to decompose, water is formed, and the carbonic acid gas remains alone combined with the metal, as happens with cyanogene, sulphur, chlore and iode, in the formation of the cyanures, sulphurets, chlorures and iodures.

A third advantage equally important is, to prepare by the natural classification of simple bodies, that of compound substances,—a work of much more labour, and to which I purpose to devote another paper. I know that the compound bodies have been already classed in a manner much more conformable to their true analogies than their elements have been. Many things have doubtless been done in this respect; but more perhaps remains to be done; and the discovery of the new substances with which the domains of chemistry have been enriched within these few years, cannot fail to lead to a modification and generalization of the principles according to which we now class compound substances, and to determine in a more precise manner the signification of the names which serve to designate the various kinds of combinations, and particularly that of the words acid, alkali, salt, &c.

I shall confine myself in this paper to the simple bodies, and shall divide them into three heads. I shall offer in the first, some general considerations on the order according to which it is proper to arrange bodies, so that this order may be as conformable as possible to their natural analogies; and on the means of avoiding the junction which has been hitherto made of the metals with bodies very different in almost all their other characters, and which have only been brought together because the energy of their affinity for oxygen is nearly the same,—a circumstance certainly remarkable, but to which perhaps too much importance has been attached,—and which certain considerations on the natural order of simple bodies, the principal results of which I shall soon detail, ought to induce us to regard as secondary, when it does not concur with other analogies which embrace the whole of the properties of the body.

Under the second head, I shall unite under natural genera the bodies which present characters of resemblance so multiplied and important that it is impossible to separate them in every classification which shall not be purely artificial; and I shall settle at the same time what places ought to be occupied in the natural order by the simple bodies which seem to form the passage from one genus to another, presenting analogies very striking with substances belonging to two different genera. In this case, they indicate between these two genera an analogy which it will perhaps be difficult to ascertain without their assistance, but which is not the less real, and according to which we ought to place them in succession after each other, so that the body which establishes the link of the chain is at the end of the first, or the beginning of the second, in order to be always between two bodies which they resemble by common characters. Nothing then remains but to determine with which of the two genera it ought to be definitively united, by comparing the properties which it shares with the one, and those which are common to it with the other, in order to decide according to the number and importance of the analogies which result from these properties. The analogies to which these researches will lead us, will fix in an invariable manner the natural order of the simple bodies, conformably to the general idea which I am about to give of them.

The last head of this paper will have for its object to examine once more the various genera into which all these bodies shall have been distributed according to the data laid down in the preceding head, in order to assign to each of them a distinctive character formed by the union of some remarkable properties, chosen in such a way that they cannot be found at once but in a body appertaining to the genus which it is wanted to characterize, and to see at the same time according to what principles of nomenclature we could, if necessary, establish for each genus a denomination common to all the bodies which form part of it.

§ I. *On the impossibility of reconciling the manner in which chemists have hitherto ranged simple bodies, and the distinctions which have been established between them; with a classification deduced from the whole of their properties; and on the order which it is proper to adopt, to unite as much as possible those which present the most characters in common.*

The first source of the artificial classifications hitherto used, seems to me to have arisen from the old distinction of the metals and non-metallic bodies. I must confess, however, that this division leads us to separate but a very small number of bodies which we ought to unite in the natural order; and that it is in general tolerably conformable to the classification which results from the comparison of all the properties of bodies; and that it will even be sufficient, in order that it may embrace none of the genera which I regard as natural, to separate from the metals three substances which are generally united with them, arsenic, tellurium, and silicium: but then it becomes very difficult to assign a character which distinguishes in every case the metals from the non-metallic bodies. Those which originally had served as the basis for this distinction can no longer be used as such, since most of the metals hitherto regarded as of that class are brittle; and because some have been discovered even lighter than water; and iode, and even carbon, when its particles are very close, as in animal charcoal, present the metallic lustre and a perfect opacity; and because chemists have discovered in carbon the property of being a conductor of the electric fluid, &c. A more important character, viz. that of producing salifiable bases on being united with oxygen, cannot be considered as sufficient for characterizing exclusively the metals; because some metals do not form any, because the boric and the nitrous acids are combined with the sulphuric acid, and because the products of those combinations have all the characters of the acid salts, which they resemble even more by their easy crystallization than several metallic solutions the oxide of which is precipitated in proportion as they are evaporated; solutions which are only considered as salts, precisely because they are compounded of an acid and of the oxide of a body which we have been accustomed to regard as a metal. This character, which is admitted besides as exclusive, will remove tellurium from arsenic, and particularly from iode; whereas the far more important property which it possesses of forming with hydrogen a permanent acid gas places it necessarily between those two bodies. The first object to which I shall turn my attention in the following article, will be to examine to what extent we might preserve the distinction of the metals, and of the non-metallic bodies, by subjecting the character which we shall choose for defining it in a precise manner, to the modification required by the necessity of rendering it conform to the natural order of simple bodies. I shall confine myself to remarking, that in the way in which it has been admitted, it has retarded the progress of the true theory of chemistry, by inducing a neglect of the observation of the properties by which certain metals are connected with the other simple bodies, and to which we cannot pay too much attention, when it is required to ascertain the truly natural order which exists between both the one and the other. The character drawn from the various degrees of affinity has still more contributed to establish between bodies, and particularly between the metals, approximations disavowed by nature. I shall confine myself to quoting in this respect, an example which appears to me very striking. Silver and gold form equally with oxygen combinations which an elevated temperature easily decomposes: from that instance those two metals have been regarded as being entitled to be placed very near each other in every methodical ar[illegible]ement of simple bodies. Nevertheless the degree of affinity for chlore ought not, in the eyes of the chemist who has precise ideas as to the action, equally energetic at least with that of oxygen, which it exercises on the metals, to be regarded as a character less important than the degree of affinity for oxygen; and the chlorure of gold is decomposable by heat; whereas that of silver, kept free from the contact of water and hydrogen, is absolutely unalterable at the highest temperature.

Here therefore we have two motives nearly equal, one for uniting and the other for separating these bodies. In order to resolve this difficulty, it is indispensable to have recourse to other properties; those of their oxides and their salts which they form with the acids, are then exhibited naturally, and decide the question, by showing us that the supposed analogy between those two metals is not confirmed by the resemblance of their principal characters. In fact, the oxide of silver is very alkaline, a little soluble in water, and completely saturates the acids: the oxide of gold presents nothing similar: and this difference, added to some other properties, less important, it is true, which silver presents in the metallic state, places this body with lead near potassium and sodium, and consequently very far from gold.

Since precise notions have been acquired as to the nature of chlore, several chemists have ceased to give to the properties which depend on the affinity of simple bodies for oxygen, an exclusive preponderance; but then one too great has been given to the assimilation which has been made of oxygen and of chlore. It has been attempted to arrange all the simple bodies in two classes, independent of each other, under the names of *combustible bodies*, and *supporters of combustion*. This division has had the same inconvenience with that of the metals and the non-metallic bodies, by making us mistake the analogies of bodies which it places in different classes. This inconvenience has even been the more injurious to the theory of chemistry, in as much as it has taught us to neglect analogies much more complete and more striking than those which the separation of the metals and other simple bodies had caused to be neglected. Such are the analogies of the chloric and iodic acids with the sulphuric and nitric acids, of the chlorates and the iodates with the nitrates, and particularly that of chlore and iode with sulphur, greater still than the analogy of the same bodies with oxygen. The combination of those four substances with the metals to which they strongly adhere, takes place by occasioning a greater or less extrication of heat, and frequently of light: the compounds which result present a crowd of common properties: so far there is no occasion to approximate more particularly chlore and iode to oxygen than to sulphur; but this last forms [illegible] iode and chlore a permanent acid gas with hydrogen: the hydro-sulphates present the greatest analogy with the hydro-iodates and hydro-chlorates: they are reduced into sulphurets, as the latter are into iodures and chlorures, when they are insoluble, or after having evaporated their solutions, and dried their residues—properties which still more closely approximate iode and chlore to sulphur than to oxygen. It is impossible to separate iode from chlore, from which it differs only because the same characters are manifested at a less degree of energy; and nevertheless, why should iode be a supporter of combustion rather than sulphur, which is united to most of the metals with a greater extrication of light and heat? Shall we say that the sulphur is combined with the oxygen of the atmospheric air when we expose it to a sufficient temperature, and that this is not the case with iode also? But is the latter not also combined with oxygen when we place it in contact with the gaseous oxide of chlore, discovered by the celebrated chemist who was the first to decompose the alkalis, and demonstrate that chlore is a simple body, conformably to the opinion already given by the French chemists as an admissible hypothesis? It is of no use to insist longer on considerations of this kind: what precedes is sufficient to show how easy it is to be led into error, and to give too much importance to certain analogies, when we commence by establishing between the bodies which we purpose to classify, general divisions founded on a single character only. We should then deceive ourselves still more, if we thought to be able to arrange the simple bodies in an order conformable to their natural analogies, by forming of them a series dependent on their various degrees of affinity for one of them, for oxygen for instance. On comparing all the properties which those substances present, we find that they form a system in which every body belongs, on one side or the other, to bodies adjoining them, by analogies so strong that we are not able to establish in any way the complete separation which will be required by the reduction of the system into a single series; so that we must represent it to ourselves as a sort of circle, in which two bodies placed at the two extremities of the chain formed by all the rest, approach and unite mutually by common characters. I have long endeavoured to establish a natural order among simple bodies, by arranging them in a single series, which should commence with those whose properties presented the most complete opposition to those of the bodies which I attempted to place at the end of the series: these attempts have not been attended with any success; and it is by them that I have been led to adopt an order quite different, of which I shall in the first place attempt to give a general idea, which shall be developed and defined in the two following articles. The bodies which have been hitherto considered as non-metallic, all possess the property of forming acids with some among them, the distinctive character of which is to make acid all combinations into which they enter in sufficient quantity. Several metals, and even some of those best entitled to this name, also present this property, and produce acids on being combined with the same acidifying substances. Those metals form two groupes, distinguished besides from each other by numerous differences. Some are eminently fixed and infusible; and it seems to be the same case with their combinations with chlore, at least if we may judge by the chlorure of chrome which presented this property to M. Dulong. It is with oxygen that they produce the acids from which the chemists have drawn the character which distinguishes them. The others are very fusible; the combinations which they form with oxygen present but feeble acid characters; for we ought not to reckon among them arsenic, which, as I have already said, ought to be united to the bodies which are considered as not metallic. And but for the labours of Messrs. Chevreul and Berzelius, the acidity of the peroxides of tin and antimony would have been still unknown; but these metals produce with chlore compounds liquid, or of a butyraceous consistency and volatile, and which possess the most essential properties of the acids. It is easy to see that the non-metallic simple bodies are united on the one hand with the infusible metals acidifiable by carbon and borium, and on the other with tin and antimony by phosphorus and arsenic, the combinations of which with chlore have the greatest analogy with the chlorostannic acid (*spiritus Libavii*). All the other metals ought therefore to be placed between tin and antimony on the one hand, and the infusible acidifiable metals on the other. Those which add to the greatest affinity for oxygen, the property of forming with it alkaline combinations, occupy in some measure the middle part of this interval, and are connected on the one hand with tin and antimony, and on the other with tungsten, columbium, chrome, and molybdenum, by two series of metals, which present in the one and the other series all the degrees of affinity for oxygen, and the oxides of which also pass gradually through the various degrees of alkalinity and acidity which characterize that kind of compounds; but the bodies of which the two series are composed, present besides sufficient differences to enable us always easily to determine that to which they belong.

Such is the order by which I have been led, not by systematic views, which I disavow; but after having made a great number of attempts, in order to see if we could not adopt another with out wandering from natural analogies, and after having compared simple bodies under every point of view which can be presented by the properties which they possess.

I shall return to the subject in another paper.

XCIII. *On the Laws observed in the Distribution of vegetable Forms.* By ALEXANDER *Count* HUMBOLDT*.

BOTANY, long confined to the simple description of the external forms of plants and their artificial classification, now presents several branches of study, which place it more on a footing with the other sciences. Such are the distribution of vegetables according to a natural method founded upon the whole part of their structure; physiology, which displays their internal organization; botanical geography, which assigns to each tribe of plants their height, limits, and climate. The terms *alpine plants, plants of hot countries, plants of the sea-shore*, are to be found in all languages, even in those of the most savage nations on the banks of the Oronoko. They prove that the attention of men has been constantly fixed on the distribution of vegetables, and on their connexion with the temperature of the air, the elevation of the soil, and the nature of the ground which they inhabit. It does not require much sagacity to observe, that on the slope of the high mountains of Armenia, vegetables of a different latitude follow each in succession, like the climates, superposed as it were upon each other. This idea of Tournefort, developed by Linnæus in two interesting dissertations, (*Stationes et Coloniæ Plantarum*,) nevertheless contains the first seeds of botanical geography. Menzel, the author of an unpublished Flora of Japan, strongly recommends to travellers researches as to the distribution of species in the different regions of the globe. He had even pointed out the result before by the name of the Geography of Plants. This appellation was again employed, and almost at the same time, about the year 1783, by the Abbé Giraud Soulavie, and by the celebrated author of the Studies of Nature; a work which, amid a great variety of very inaccurate ideas as to the *physique* of the globe, contains some profound and ingenious views as to the forms, relations, and habitudes of vegetables. Abbé Giraud Soulavie was occupied in preference with the plants already cultivated: he has distinguished the climates of the olive trees, the vines, and the chesnuts. He gives a vertical section of Mount Mezin, to which he has added the barometrical heights, "because," as he says, "he has a great contempt for every result taken from barometrical measurement." His Geography of the Plants in the South of France was followed by the *Tentamen Historiæ geographicæ Vegetabilium* of the learned Professor Strohmayer, published in 1800 at Gottingen in the form of a dissertation; but this Tentamen exhibits rather the plan of a future work, and the catalogue of authors to be consulted, than information respecting the altitudes which spontaneous plants reach in different climates. The case is the same with the very philosophical views announced by M. Treviranus in his *Essai de Biologie;* we therein find general considerations, but no measurements of heights, and no thermometrical indications, which are the solid bases of the geography of plants. This study has not risen to the rank of a science, until men of science have perfected both the measures of heights by barometrical observations, and the determination of mean temperatures; or, what is more important for the development of vegetation, the determination of the differences between the temperature of summer and winter and between that of day and night. Few branches of study have in our day made more rapid progress; and a long time has not intervened between the first efforts and the present period, when by the united observations of a great number of travellers, we have succeeded in fixing the limits of vegetables in Lapland, the Pyrenees, the Alps, Caucasus, and the Cordilleras of America.

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* Extracted from a paper read to the French Institute, Feb. 5, 1816.

The vegetables which cover the vast surface of the globe present, when we study by natural classes or families, striking differences in the distribution of their forms: it is to the laws of this distribution that I have recently turned my attention. On limiting them to the countries in which the number of the species is exactly known*, and by dividing this number by that of the *Glumaceæ* †, the leguminous plants, the labiated, and the compound, we find numerical relations which form very regular series. We see certain forms become more common from the equator towards the pole, like the ferns, the glumaceæ, the ericineæ, and the rhododendrons. Other forms on the contrary increase from the poles towards the equator, and may be considered in our hemisphere as southern forms: such are the rubiaceæ, the malvaceæ, the euphorbia, the leguminous and the composite plants. Finally, others attain their maximum even in the temperate zone, and diminish also towards the equator and the poles. Such are the labiated plants, the amentaceæ, the cruciferæ, and the umbelliferæ. Part of these data long since struck botanical travellers, and all those who have looked into herbals. It was known that the cruciferæ and umbelliferæ disappeared almost entirely in the plains of the torrid zone, and that none of the malvaceæ were found beyond the polar circle. It is the same with the geography of the plants as with meteorology. The results of those sciences are so simple that in all ages general ideas have been formed of them: but it is only after laborious researches, and after having collected a great number of accurate observations, that numerical results were attained, and an acquaintance with the partial modifications undergone by *the law of the distribution of forms.* A table which we have drawn up exhibits this law with respect to sixteen families of plants distributed over the equatorial, temperate, and glacial zones. We there see with satisfaction mixed with surprise, how in organic nature, the forms present constant relations under the same *isothermal parallels*, *i. e.* on curves traced by points of the globe which receive an equal quantity of heat. The grasses form in England 1-12th, in France 1-13th, in North America 1-10th, of all the phanerogamous plants. The glumaceæ form in Germany 1-7th; in France 1-8th; in North America 1-8th; in New Holland, according to the researches of Mr. Brown, 1-8th; of the known phanerogamous plants. The composite plants increase a little in the northern part of the new continent; for, according to the new Flora of Pursch, there is between the parallels of Georgia and Boston 1-6th; whereas in Germany we find 1-8th; and in France 1-7th, of the total number of the species with visible fructification. In the whole temperate zone, the glumaceæ and the composite plants form together, nearly one-fourth of the phanerogamous plants; the glumaceæ, the compositæ, the cruciferæ and the leguminosæ, together nearly one-third. It results from these researches that the forms of organized beings are in a mutual dependence, and that the unity of nature is such that the forms are limited, the one after the other, according to constant laws easy of determination. When we know upon any point of the globe the number of species presented by one of the great families of the glumaceæ, the composite, the cruciferous, or the leguminous plants, we may estimate with considerable probability both the total number of the phanerogamous plants, and the number of species which compose the other vegetable families. It is thus that, by knowing under the temperate zone the number of the cyperaceæ or composite plants, we may guess at that of the gramineous or leguminous plants.

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* Lapland, France, England, &c. according to Messrs. Wahlenberg, Buch, Ramond, Decandolle, and Smith.

† The Glumaceæ contain the three families of *Gramineæ*, *Cyperaceæ*, and *Juncaceæ*.

The number of vegetable species described by botanists, or existing in European herbals, extends to 44000, of which 6000 are agamous. In this number we had already included 3000 new phanerogamous species enumerated by M. Bonpland and myself. France, according to M. Decandolle, possesses 3645 phanerogamous plants, of which 460 are glumaceæ, 490 composite,[*] and 230 leguminous, &c. In Lapland there are only 497 phanerogamous plants; among which are 124 glumaceæ, 58 composite, 14 leguminous, 23 amentaceous, &c. See my Essay on the Geography of Plants published in 1806, and of which I am preparing a new edition.

In order to account for the differences which exist sometimes between the relations exhibited by Germany, North America, and France, we must take into consideration the more or less temperate climates of those regions. France extends from 42½° to 51° of latitude. On this extent the mean annual heat is 16° 7′ to 11°: the mean heats of the summer months are 24° to 19°. Germany, comprised between 46° and 54° of latitude, presents at its extremities mean annual temperatures of 12° 5′ and 8° 5′. The mean heats of the summer months there are 21° and 18°. North America, in its immense extent, presents the most varied climates. Mr. Pursch has made us acquainted with 2000 phanerogamous plants which grow between the parallels of 35° and 44°; consequently under mean annual temperatures of 16° and 7°. The Flora of North America is a mixture of several Floras. The southern regions give it an abundance of malvaceæ and composite plants; the northern regions, colder than Europe under the same parallel, furnish to this Flora abundance of rhododendrons, amentaceæ, and coniferæ. The caryophylleæ, the umbelliferæ, and the cruciferæ are in general more rare in North America than in the temperate zone of the old continent[*].

These constant relations observed on the surface of the globe, in the plains from the equator to the pole, are again traced in the midst of perpetual snows on the summits of mountains. We may admit, in general, that on the cordilleras of the torrid zone the boreal forms become more frequent. It is thus that we see prevail at Quito on the summit of the Andes, the ericineæ, the rhododendrons, and the gramineous plants. On the contrary, the labiatæ, the rubiaceæ, the malvaceæ, and the euphorbiaceæ then become as rare as they are in Lapland. But this analogy is not supported in the ferns and the composite plants. The latter abound on the Andes, whereas the former gradually disappear when they rise above 1800 fathoms in height. Thus the climate of the Andes resembles that of northern Europe only with respect to the mean temperature of the year. The repartition of heat into the different seasons is entirely different, and powerfully influences the phænomena of vegetation. In general, the forms which prevail among the Alpine plants are, according to my researches, UNDER THE TORRID ZONE, the *gramineæ* (ægopogon, podosæmum, deyeuxia, avena); the *compositæ* (culcitium, espeletia, aster, baccharis); and the *caryophylleæ* (arenaria, stellaria). UNDER THE TEMPERATE ZONE, the *compositæ* (senecio, leontodon, aster); the *caryophylleæ* (cerastium, cherleria, silene); and the *cruciferæ* (draba, lepidium). UNDER THE FROZEN ZONE, the *caryophylleæ* (stellaria, alsine); the *ericineæ* (andromeda) and the *ranunculaceæ*.

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[*] For the sake of those who are not much conversant in descriptive botany, we shall here enumerate the plants which serve as a type to the forms or principal families: *Glumaceæ*, rushes, tares; *orchideæ*, orchis, satyrion, vanilla; *labiatæ*, sage; *cricineæ*, broom; *compositæ*, coltsfoot, tussilago; *rubiaceæ*, madder, quinquina; *umbelliferæ*, fennel; *cruciferæ*, ra[illegible]abbage; *malvaceæ*, cotton; *leguminosæ*, furze, truffles, sensitiv[illegible]; *euphorbiaceæ*, milky thistle; *amentaceæ*, willow, oak; *coniferæ*, pi[illegible] juniper.

These researches into the law of the distribution of forms, naturally lead to the question whether there exist plants common to both continents? a question which inspires the more interest, as it belongs to one of the most important problems in Zoonomia. It has been long known, and it is one of the most interesting results from the *geography of animals*, that no quadruped, no terrestrial bird, and, as appears from the researches of M. Latreille, almost no insect, is common to the equatorial regions of the two worlds. M. Cuvier is convinced by precise inquiries that this rule applies even to reptiles. He has ascertained that the true boa constrictor is peculiar to America, and that the boas of the old continent were pytons. As to the regions beyond the tropics, Buffon has multiplied beyond measure the number of the animals common to America, to Europe, and the north of Asia. We are assured that the bison, the stag, and the goat of America, the rabbit and the musk rat, the bear, &c. &c. are species entirely different from those of Europe, although Buffon had affirmed the contrary. There remain only the glutton, the wolf, the white bear, the red fox, perhaps also the elau, which have not characters sufficient to entitle them to be specific. Among the plants, we must distinguish between the agamæ and the cotyledoneæ; and by considering the latter between the monocotylodens and the dicotyledons. There remains no doubt that many of the mosses and lichens are to be found at once in equinoctial America and in Europe: our herbals show this. But the [illegible] is not the same with the vascular agamæ as with the [illegible] of a cellular texture. The ferns and the lycopodiaceæ do not follow the same laws with the mosses and the lichens. The former, in particular, exhibit very few species universally to be found; and the examples cited are frequently doubtful. As to the phanerogamous plants (with the exception of the rhizophora, the avicennia, and some other littoral plants), the law of Buffon seems to be exact with respect to the species furnished with two cotyledons. It is absolutely false, although it has been often affirmed, that the ridges of the cordilleras of Peru, the climate of which has some analogy with the climate of France or Sweden, produce similar plants. The oaks, the pines, the yews, the ranunculi, the rose-trees, the alchemilla, the valerians, the stellaria, the draba of the Peruvian and Mexican Andes, have nearly the same physiognomy with the species of the same genera of North America, Siberia, or Europe. But all these alpine plants of the Cordilleras, without excepting one among three or four thousand which we have examined, differ specifically from the analogous species of the temperate zone of the old continent. In general, in that part of America situated between the tropics, the monocotyledontal plants alone, and among the latter almost solely the cyperaceæ and the gramineæ, are common to the two worlds. These two families form an exception to the general law which we are here examining,—a law which is so important for the history of the catastrophes of our planet, and according to which the organized beings of the equinoctial regions differ essentially in the two continents. I have given in my *Prolegomena* a precise catalogue of those monocotyledontal plants common to the shores of the Oronoko, Germany, and the East Indies. Their number does not exceed 20 or 24 species, among which it is sufficient to cite the cyperus mucronatus, c. hydra, hypælyptum argenteum, poa eragrostis, andropogon, allioni, &c.

In North America placed beyond the tropics, we find nearly one-seventh of monocotyledontal and dicotyledontal plants common to the two continents. Of 2900 phanerogamous species collected in the New Flora of Pursch, 390 are European. It is true that we may hazard some doubts, as well with respect to the number of the plants which have accompanied Europeans from one hemisphere to the other, as upon those which, when better examined, will be recognised subsequently as new species: but it is impossible that this state of uncertainty should extend to all; and it is to be presumed that, even after a careful examination, the number of the species common to the temperate zones of the two worlds will still remain very considerabl[illegible]nalogous. Mr. Brown recently undertook some researches [illegible] the plants of New Holland. A twenty-eighth part of all the [illegible]cotyledons hitherto found in the austral continent are common to it with England, France, and Germany. Among the dicotyledons the ratio is only 1 in 200; which proves once more how, in the two hemispheres, the grasses and the cyperaceæ are the most diffused, on account of the extreme flexibility of their organization. It would be desirous that learned zoologists should endeavour to examine the analogous numerical ratios presented by the distribution of the different families of animals on the globe.

In the austral hemisphere the vegetable forms of the torrid zone advance more towards the pole than in the boreal hemisphere. The fern-trees in Asia and America are rarely to be found beyond the tropic of Cancer; whereas in the austral part of our globe the *Dicksonia antarctica*, the trunk of which is six metres in height, pushes its migrations as far as Van Diemen's Land under the latitude of 42°: it has even been found in New Zealand, in Dusky Gulph, under the parallel of Lyons.

Other forms not less majestic, and which were thought to belong exclusively to the equinoctial Flora, the parasite orchideæ (epidendra, dendrobia) are found mixed with the arborescent ferns far beyond the tropic of Capricorn, in the centre of the austral temperate zone. These phænomena of the geography of plants prove how vague is what has been generally said of the great diminution of temperature in the southern hemisphere, without distinguishing between the parallels more or less near the pole, and without any regard to the division of heat among the different seasons of the year. Those regions towards which the equinoctial forms extend, possess, on account of the immensity of the seas which surround them, a true *island climate.* From the tropic of Capricorn to the parallel of 34°, and perhaps still further, the mean heats of the year (*i. e.* the quantity of heat received by any given point of the globe) do not differ considerably in the two hemispheres. On casting our eyes over the three continents, New Holland, Africa, and America, we find that the mean annual temperature of Port Jackson (lat. 33° 51′) is 19° 3′ of the centigrade thermometer: that of the Cape of Good Hope (lat. 33° 55′) 19° 4′; that of the town of Buenos Ayres (lat. 34° 36′) 19° 7′. We may be surprised at this great equality in the distribution of heat by the 34° of austral latitude. Meteorological observations, still more precise, prove that in the boreal hemisphere, under this very parallel of 34°, we find a mean temperature of 19° 8′. On advancing towards the antarctic pole, perhaps even to the parallel of 57°, the temperatures of the two hemispheres differ less in winter than summer. The Malouine Islands situated in 51° and a half of south latitude, have less intense cold in winter than is experienced at London. The mean temperature of Van Diemen's Land seems to be 10°; it freezes during winter, but not so much as to destroy the fern-trees and the parasite orchideæ. In the adjoining seas Capt. Cook, in 42° of austral latitude, did not see the thermometer fall below +6°, 6 in the midst of winter (July). To these very mild winters, summers succeed remarkable for an extraordinary coolness. At the southern extremity of New Holland (lat. 42° 41′) the temperature of the air rarely rises in the midst of summer at noon-day higher than 12° or 14°; and in Patagonia, as in the adjoining ocean (lat. 48°—58°), the mean heat of the warmest month is only 7°—8°; whereas in the boreal hemisphere at Petersburg and Umeo (lat. 59° 56′ and 63° 50′) this heat exceeds 17—19. It is this mild temperature of the islands, which the southern countries enjoy between 30° and 40° of latitude, which permits the vegetable forms to pass beyond the tropic of Capricorn. They embellish a great part of the temperate zone; and the genera which the inhabitant of the northern hemisphere regards as exclusively belonging to the tropical climates, present numerous species between the parallels of 35° and 38° of south latitude.

XCIV. *Notices respecting New Books.*

Essays on Insanity, Hypochondriasis, and other Nervous Affections. By JOHN REID, *M.D. Member of the Royal College of Physicians, London; and late Physician to the Finsbury Dispensary.* pp. 272. 8vo. Longman and Co.

OUR readers will find in the above work a rational and philosophical view of the most distressing of all the afflictions to which human nature is subject. The treatment recommended by Dr. Reid is that of gentleness and kindness on all occasions, and coincides in all its bearings with the views which have been taken of this distressing subject, by those enlightened legislators and other friends of humanity who have of late directed the public attention to the condition of those afflicted with mental derangement.

Dr. Reid's ideas of modern lunatic asylums are precisely those which now generally prevail, and by the adoption of which much human misery will in all probability be in future spared; and to his credit be it remembered, that the hints which we are about to subjoin were thrown out in another form several years ago by the enlightened author, and so far he may claim the merit of originality. We now present our readers with the doctor's description of those charnel houses of the human intellect called Lunatic Asylums, as a specimen not only of his philosophical and expanded views, but as a pleasing example how forcibly such a heart-rending subject can be made to press on the human mind, and even how attractive it can be rendered, by those charms of eloquence, accompanied by a poetical yet chaste imagination, for which all Dr. Reid's writings are distinguished.

LUNATIC ASYLUMS.

> "I am not mad! I have been imprisoned for mad—scourged for mad—banished for mad—but mad I am not."
>
> *Guy Mannering.*

"The mind of a man may be bruised or broken as well as any limb of his body, and the injury, when it occurs, is not so easy of reparation. A morbidly tumid fancy cannot, like many other swellings, be made speedily to subside; an intellect out of joint will not allow of being set with the same facility as a dislocated bone; nor can the deep and often hidden ulcerations which arise from mental distemper or disorganization, be healed with the same readiness or certainty as those more palpable sores which take place on the surface of the body. On this account it is, that so close and vigilant an observation is required in watching the incessantly varying movements, and in inspecting the too exquisitely delicate texture of a disordered and highly wrought imagination.

"One thing at least is certain, that in the management of such maladies, tenderness is better than torture, kindness more effectual than constraint. Blows, and the strait-waistcoat, are often, it is to be feared, too hastily employed. It takes less trouble to fetter by means of cords, than by the assiduities of sympathy or affection. Nothing has a more favourable and controuling influence over one who is disposed to or actually affected with melancholy or mania, than an exhibition of friendship or philanthropy; excepting indeed in such cases, and in that state of the disease, in which the mind has been hardened and almost brutalized, by having already been the subject of coarse and humiliating treatment. Where a constitutional inclination towards insanity exists, there is in general to be observed a more than ordinary susceptibility to resentment at any act that offers itself in the shape of an injury or an insult.

"Hence it will not appear surprising, that as soon as an unfortunate victim has been inclosed within the awful barriers of either the public or the minor and more clandestine Bethlems, the destiny of his reason should, in a large proportion of cases, be irretrievably fixed. The idea that he is supposed to be insane, is almost of itself sufficient to make him so; and when such a mode of management is used with men, as ought not to be, although it too generally is, applied even to brutes, can we wonder if it should often, in a person of more than ordinary irritability, produce, or at any rate accelerate the last and incurable form of that disease, to which at first perhaps there was only a delusive semblance or merely an incipient approximation?

“ Tasso, the celebrated poet, was once instigated by the violence of an amorous impulse to embrace a beautiful woman in the presence of her brother, who happening to be a man of rank and power, punished this poetic license by locking up the offender in a receptacle for lunatics. It is said that by this confinement he was made mad, who was before only too impetuous or indiscreet.

“ That a wretched being, who has been for some time confined in a receptacle for lunatics, is actually insane, can no more prove that he was so when he first entered it, than a person's being affected with fever in the black hole of Calcutta, is an evidence of his having previously laboured under febrile infection.

“ Bakewell, the late celebrated agriculturist, was accustomed to conquer the insubordination or any vicious irregularity of his horses, not by the ordinary routine of whipping and spurring, but by the milder and more effectual method of kindness and caresses; and it is worthy of being remembered and practically applied, that, although the human has higher faculties than other animals, they have still many sympathies in common; there are certain laws and feelings which regulate and govern alike every class and order of animated existence.

“ In order to obtain a salutary influence over the wanderings of a maniac, we must first secure his confidence. This cannot be done, without behaving towards him with a delicacy due to his unfortunate state, which for the most part ought to be regarded not as an abolition, but as a suspension merely of the rational faculties. Lord Chesterfield speaks, in one of his humorous essays, of a lady whose reputation was not lost, but was only *mislaid*. In like manner, instead of saying of a man that he has lost his senses, we should in many instances more correctly perhaps say that they were mislaid. Derangement is not to be confounded with destruction; we must not mistake a cloud for night, or fancy, because the sun of reason is obscured, that it will never again enliven or illuminate with its beams. There is ground to apprehend that fugitive folly is too often converted into a fixed and settled phrensy; a transient guest into an irremovable tenant of the mind; an occasional and accidental aberration of intellect, into a confirmed and inveterate habit of dereliction; by a premature and too precipitate adoption of measures and methods of management, which sometimes, indeed, are necessary, but which are so only in cases of extreme and ultimate desperation.

" A heavy responsibility presses upon those who preside or officiate in the asylums of lunacy. Little is it known how much injustice is committed, and how much useless and wantonly inflicted misery is endured, in those infirmaries for disordered, or rather cemeteries for deceased intellect. Instead of trampling upon we ought to cherish, and by the most delicate and anxious care strive to nurse into a clearer and a brighter flame the still glimmering embers of a nearly extinguished mind.

" It is by no means the object of these remarks to depreciate the value of institutions which, under a judicious and merciful superintendance, might be made essentially conducive to the protection of lunatics themselves, as well as to that of others, who would else be continually exposed to their violence and caprice. But it is to be feared that many have been condemned to a state of insulation from all rational and sympathizing intercourse before the necessity has occurred for so severe a lot. Diseased members have been amputated from the trunk of society before they have become so incurable or unsound as absolutely to require separation. Many of the depots for the captivity of intellectual invalids may be regarded only as nurseries for and manufactories of madness; magazines or reservoirs of lunacy, from which is issued, from time to time, a sufficient supply for perpetuating and extending this formidable disease,—a disease which is not to be remedied by stripes or strait-waistcoats, by imprisonment or impoverishment, but by an unwearied tenderness, and by an unceasing and anxious superintendance.

" The grand council of the country ought to be aroused to a critical and inquisitorial scrutiny into the arcana of our medical prisons, into our *slaughter-houses* for the destruction and mutilation of the human mind."

Mr. Sowerby announces the continuation of his " British Mineralogy." He intends to complete it in three or four more numbers, and to give copious indexes to the whole work. He begs leave to express a hope that his friends will assist him in making it as complete as possible.

XCV. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

June 13.—T. A. Knight, Esq. F.R.S. in a letter to the President, communicated the results of some further experiments which he has made on the leaves of plants, tending to prove that the matter of timber is formed in them, and that the true sap, descending from the leaves to the roots, forms alburnum. He made a transverse and longitudinal section of the leaf stalk of a vine, and raising up the bark an inch below the leaf, inserted it in the section of the leaf stalk; and thus preserving a communication between the bark and leaf, the intermediate part being rolled tight with paper, he found that the bark acquired thickness, and a woody character. He next took the leaf of a potatoe and planted it; but although it did not, as he expected, produce tuberous roots, it formed, like cuttings of shrubs previous to their striking root, a large round lump, which lived through the winter, and he is now trying whether it will grow into a perfect potatoe. He took a shoot of a vine detached from the stock, and immersing a part of its largest leaf in water during a month, the smaller leaf not only lived but grew, and acquired thickness: this he considers a clear proof that the smaller leaf was nourished and augmented only by the food it received from the large one, which was partly in water. In addition to these experiments Mr. Knight observes, that if trees be deprived of their leaves their fruit never ripens; that evergreens bear fruit at all seasons of the year, in winter as well as summer; and that deciduous plants only produce their fruit previous to shedding their leaves. The common holly is seen with its berries in the midst of winter. The quality and quantity of fruit, he seems to think, almost entirely depend on the nature and quality of the leaves; and hence the necessity of gardeners being more cautious in stripping off the leaves of fruit trees where they tend to keep the sun off the fruit.

Dr. Holland, F.R.S. communicated an account of a manufactory of sulphat of magnesia in Monte della Guardia, about five miles from Genoa. This mountain, which is at the extremity of the Appenines, is about 2000 feet above the level of the sea, and abounds in veins of copper and iron pyrites, and magnesian limestone. The original manufactory was confined to sulphat of iron and copper; but it was discovered that magnesia was very abundant, and that it also could be procured by the same process. The process is very simple; the ore is roasted eight or ten days with a wood fire, it is then dissolved in water, and the sulphats of iron and copper crystallized; about one per cent. of the magnesian lime is added, and sulphat of magnesia is formed, which is sold in Italy under the denomination of *sal inglesa.* The peculiarity in this process is the quantity of magnesian lime added: if it were greater, sulphat of lime would be produced; if less, no sulphat of magnesia would be formed. This manufactory is but small, and is situated on the side of the hill, about 1600 feet above the level of the sea. The ore is dug out of the mountain in a manner to form a kind of tunnel, but the magnesian limestone is very abundant.

June 20.—Dr. Brewster, in a letter to the President, gave an account of his experiments on the eyes of fishes, by which it appears that the crystalline lens of fishes, according to its density, is capable of receiving the property of double refracting crystals, the same as glass, by compression. Dr. B. related a number of experiments which he made on the eyes of fishes, and their effects on colours; from which he concludes that one part of a fish's eye experiences mechanical dilatation, and another mechanical contraction. He thinks that the chief cause why so little progress has been made in augmenting our knowledge of the eye and of vision, is owing to an excessive confidence in a supposed analogy between optic glasses and the crystalline lens.

Sir Everard Home, Bart. furnished a supplement to a former paper on the skeleton of a peculiar kind of fish in Mr. Bullock's Museum. Two gentlemen having procured parts of this singular animal in different places of the country, were so kind as to submit them to Sir E.'s inspection; in consequence of which he can now state with confidence, what he before only conjectured, that the animal must have been a fish, as the bones which are wanting to complete Mr. Bullock's specimen have been partly discovered in Dorsetshire, and part elsewhere. He has now made drawings of the perfect skeleton, to illustrate this and his preceding paper on the subject. He noticed particularly the characteristic differences between the ribs of land and sea animals; the former being joined to the vertebræ, in order to rise and fall in breathing; the latter being fixed, and adapted to support the sides against the lateral pressure of, and suit the fish's motion through, the water.

LINNEAN SOCIETY.

Friday, May 24.—On this day the Anniversary Meeting of the Linnean Society of London was held at the Society's house in Gerrard-street, Soho, for the Election of a Council and Officers for the present year, when the following Members were declared to be of the Council, viz.

Sir James Edward Smith, Knt. M.D.
Samuel, Lord Bishop of Carlisle.
Edward Forster, Esq.
George Bellas Greenough, Esq.
William Kent, Esq.
Aylmer Bourke Lambert, Esq.
William Horton Lloyd, Esq.
William George Maton, M.D.
Daniel Moore, Esq.
Rev. Thomas Rackett.
Joseph Sabine, Esq.
John, Lord Bishop of Salisbury,
Edward, Lord Stanley.
Thomas Thomson, Esq.

And the following were declared to be the Officers for the present year, viz.

Sir James Edward Smith, Knt. M.D. President.

Samuel, Lord Bishop of Carlisle,
Aylmer Bourke Lambert, Esq. } Vice-Presidents.
Edward, Lord Stanley,
William George Maton, M.D.
Edward Forster, Esq. Treasurer.
Alexander MacLeay, Esq. } Secretaries.
Mr. Richard Taylor,

The Members of the Society afterwards dined together at the Freemasons' Tavern, Great Queen-street, according to annual custom.

June 4.—A. B. Lambert, Esq. Vice-President, in the chair. Read part of a Monograph of the British Roses, by Joseph Woods, Esq. F.L.S.

June 18.—W. G. Maton, M.D. Vice-President, in the chair. Read a part of "Observations on the Linnæan Junci growing in Great Britain," by J. E. Bicheno, Esq. F.L.S.

Adjourned to the 5th of November.

ROYAL INSTITUTE OF FRANCE.

Analysis of the Labours of the Class of Mathematical and Physical Sciences for the Year 1815. *By M.* CUVIER.

[Continued from p. 394.]

CHEMISTRY.

The third volume of the Elementary Chemistry of M. Thenard has appeared. This learned professor therein treats at full length, and according to the most modern discoveries, (among which there are a great many for which science is indebted to him,) of the immediate principles of organized bodies, of the various productions, of their decomposition, and of their employment in the arts. The fourth volume, which will conclude the work, is in the press.

MINERALOGY AND GEOLOGY.

Among the questions which the learned who are occupied with the theory of the earth generally agitate, there are few more difficult, or which have occasioned longer and more obstinate disputes, than that of the origin of basalts, a kind of rock which some consider as the production of ancient volcanoes, while others regard them as deposited in the general liquid in which the common rocks are formed, and as analogous to the trapps of the primitive earths.

M. Cordier, inspector of mines, and correspondent of the Class, having also directed his attention to this grand problem, has imagined, in order to resolve it, means entirely new.

His first reflections made him perceive that the greatest difficulty, in order to compare the matter of a contested nature with those of which the origin, either volcanic or non-volcanic, is incontestable, is to be ascribed to the circumstance of both being often composed of particles so mixed, and reduced into a paste of an appearance so homogeneous, that it is impossible to discern them with the eye. Chemistry cannot here come to the assistance of the senses, because it confounds all those particles in its analysis, and only gives as a result the sum total of their primitive elements, in place of distinguishing those which belong to each of their species.

M. Cordier, therefore, contrived a new mode of mechanical analysis, which consists in reducing in the first place into small bits, the mineral specimens the existence of which we may suspect in rocks which we wish to examine; 2dly, determining clearly the physical characters of those parcels, and their manner of acting when exposed to the blowpipe; 3dly, pulverizing the rocks submitted to examination; and lastly, in washing and sifting the various particles detached by this pulverization, and submitting them to the same tests to which parcels of substances well known are subjected.

This is, as may be seen, a kind of microscopical mineralogy, from which M. Cordier has derived great advantage. The pastes, known by the name of lavas, and historically stated as such, were easily detected by this new analysis: their particles were easily separated: they exhibited but a small number of combinations, in which sometimes feldspar prevailed, sometimes pyroxene, and in which they were alloyed in various proportions with the ore of iron denominated *titanium*—with those three elements are mixed, but in a manner less general, amphibole, amphigene, mica, peridot, and oligistous iron.

The basaltic pastes of an origin more or less contested were as easily divided into their constituent parts, and those parts were not found different. All those ancient or modern pastes, whether recognised as lava or not, are therefore, according to the author, microscopical granites, in which the uniformity of the intermixed texture is interrupted only by very small vacancies a little less rare in some lavas than in others, and which appeared to the naked eye to be homogeneous masses, in which are prevalent either the characters of the pyroxene or those of feldspar, and which cannot then be distinguished but by two sorts.

A part of the *scoriæ* which accompany the stony lavas, and which are the first products of the coagulation of matters in fusion, are also composed of various grains, but finer, less regularly interwoven, and nevertheless of the same species with the masses which they cover; another part more changed by the action of fire approaches more the vitrified state; others, finally, are completely in this state; but there always remain so many traces of their origin that we cannot mistake them. They are always referred to one of the two principal orders of combinations recognised among the stony lavas.

M. Cordier endeavours to explain, by the difference in the state of the scoriæ, the phænomenon which has astonished so many travellers; namely, that certain currents of lava remain constantly sterile, while others are soon covered with the most luxuriant vegetation. It arises from the circumstance of the former being more vitrified than the latter, and therefore being less easily decomposed.

The author also examines the *obsidians*, or volcanic glass; and by comparing together all the shades of their greater or lesser vitrification, he always finds some traces of that pyroxene or feldspar, the predominant principles of the two orders of lava; and the obsidians which melt into black glass have shown perfect transitions even the length of the thickest basalt; in a word, the obsidians, the scoriæ, the lava, the basalts do not differ in composition, but only by the accidents in their texture. Even the volcanic sands and ashes yield upon being washed the same materials the aggregation of which forms the adjacent lavas. M. Cordier has pursued those materials into various substances, and after they had been altered by time, and extricated them from the new substances which enveloped them, or which had been as if filtered into the interstices. In a word, he has not neglected the examination of any of the modifications of the true or contested volcanic productions, and has no where found his general rules defective: but when he passed afterwards to those trapps, and petro-silex, in short, to those ancient rocks to which basalts are sometimes referred, he has no longer recognised any of those characters so marked, which are said to establish between the lavas and the basalts incontestable relations.

The mass of those ancient rocks has no apparent vacuities, and they do not differ from each other in point of colour. This cannot be insulated, nor can a mechanical analysis be made of them: consequently, if a part of those rocks are composed of heterogeneous materials, it is not possible to determine the mineralogical species to which those materials belong.

Their chemical analysis also gives other results, particularly because it yields no titanium.

Thus the pretended analogy between the trapps and the basalts will not support a rigorous examination.

As to the origin of the lavas and the causes of their fusion, M. Cordier risks no conjecture; but, considering their mass as coagulated by an instantaneous crystallization, he easily resolves the peculiar problem long debated upon: If the crystals in the lavas have been taken up completely formed from the bowels of the earth, and enveloped by them, or if they are formed afterwards in their vacant spaces or cells; or, finally, if they have crystallized at the same moment when the rest of their mass was hardened: and he gives us to understand that it is the latter opinion which he adopts.

He terminates his curious investigation by a methodical enumeration of basalts and other products of volcanoes arranged according to their materials of aggregation, and under the banners of the two substances which therein predominate, feldspar and pyroxene.

This mysterious nature of volcanoes, these immense *foci* of heat, far removed from all the conditions which keep up heat at the surface of the earth, will be still a long time one of the great objects of the curiosity of natural philosophers, and will excite their efforts so long as any hopes of success remain. A young mineralogist as zealous as he is learned, M. Mesnard de la Groye, having had occasion in 1812 and 1813 to observe several of the phænomena of Vesuvius, drew up a journal of them with great accuracy, intermixed with many original suppositions and ideas.

Since the enormous diminution which the cone of the volcano underwent in 1794, when it sunk more than 400 feet, all the eruptions have taken place from its summit; which seems to have prevented them from being so abundant and so destructive as those which issued from its sides. The bottom of the crater rose, and it is not unlikely that it will be filled.

The rivers of lava are the less abundant if a great quantity of scoriæ and small stones are thrown out during the eruption. The whole cone is covered with those small stones, which are soon changed by the acid vapours, and assume those lively and variegated colours which make them look like bunches of flowers at a distance, and which have inclined naturalists to suppose that the crater is filled with sulphur; which is so far from being true, that it is even very rare that sulphurous vapours are perceived in it: on the contrary, there rise strong and continual exhalations of muriatic acid, and sea salt is every where concreted throughout.

M. Mesnard de la Groye thence takes occasion to divide volcanoes into two classes; those in which sulphur performs an essential part, and those in which the muriatic acid prevails. It is among the latter that he classes Vesuvius.

He also notices the continual smoke which rises from the rivers of lava, and which announce great humidity. This smoke is in fact purely aqueous. No flames are seen, but sands and burnt stones; and the reverberation of the internal furnaces on the vapours which issue, causes this illusion. The lava flows very slowly: its edges when cooled form an embankment for it, and keep it above the level of the soil, which is covered with scoriæ; it is very difficult to get a sight of its fluid parts. We know besides, that its heat has nothing in it similar to that of glass in fusion; for when it envelops trunks of trees, it does not char them to the centre. M. de la Groye is also of opinion that the lava owes its fluidity to some principle which is consumed by the very act of fusion, and to this circumstance is owing the difficulty of fusing again that which has once cooled. The full mass, the part not swelled up into scoriæ, has a stony aspect: this is what the Germans call *graustein*. The author compares the periods of the fusion of the lavas with those through which the salts pass, which fuse after being swelled up. He relates some curious facts with respect to the prodigiously long duration of their heat, and thence concludes that they bear within themselves the principle of their own heat, and that they do not possess a heat simply communicated. To all these remarks M. de la Groye adds a very detailed account of the grand eruption of 1813, which produced an infinity of ashes and small stones, but the lava of which did not reach the length of the cultivated grounds.

After having studied with so much care the burning volcanoes, M. de la Groye wished also to give an account of the motives upon which the opinion is founded, that various mountains may be classed among burnt-out volcanoes; and he visited one of them which De Saussure and other great geologists had already placed in this class, but with respect to which the obstinate Neptunists will still find abundance of pretexts for confirming their doubts.

This mountain was that of Beaulieu, about three leagues from Aix in Provence. The inequalities of the soil which surrounds it, represent currents similar to those of lava: its extent is 1200 fathoms by 6 or 700 in breadth: its mean elevation above the sea is 200; the surrounding soil is calcareous to an infinite distance: towards the east are the basaltic rocks which seem to form the nucleus of the whole system, but in the basaltic part itself there are also sea-shells and abundance of limestone. The amygdaloids and the basalts are covered with them in several places; in others their fragments are incrusted with them, and compose with this limestone a sort of *breche*. He has often penetrated into the cells of the amygdaloids.

Nevertheless the principal rock is the secondary grunstein of the Germans composed of feldspar and pyroxene, sometimes in such large grains that it resembles granite. It forms a long current, and we pass from this rock by intermediate ones, comparable to trapps properly so called, to the common basalt, containing frequently peridot, and some parts of which De Saussure saw divided into prisms. There is also some *vacque* or wack which serves as the basis of the amygdaloid, and which when its cellules are empty completely resembles a porous lava; but in which they are most frequently filled with limestone, as in the mandelstein of the Germans. Lastly, we find a basaltic tufa filled with small calcareous buttons, and containing pyroxenes, peridots, micas, and those other mineral species so common in the lavas. M. Mesnard saw at Beaulieu a hollow which appeared to him like the remains of a crater. He concludes therefore, after some general reasoning against the objections of the Neptunists, that this mountain was the production of a submarine eruption, and that the sea in which it was made continued for a long time afterwards to deposit limestone. De Saussure had already appeared to be favourable to this opinion. M. Faujas St. Fond regarded it as incontestable, and M. Mesnard thought he saw in it a method of conciliating all opinions on the pretended secondary trapps, which have been so long a subject of dispute.

Among those numerous fragments of unknown organization which fill the strata of the earth, there are found impressions of an animal of a singular form composed of a sort of corslet, and an abdomen formed of several segments, each of which is divided into three lobes. Naturalists have given them the names of *entomolites* and *trilobites;* but they have not sufficiently distinguished them from each other, and have not determined to what order of stratification each species belonged.

M. Brongniart, the manager of the manufactory of Sevres, has presented a work on this subject, in which, after an exact comparison of the specimens which he procured, as well as of the descriptions and figures left by preceding authors, he shows that there are at least seven species of these trilobites, that their principal forms are sufficiently distinct to divide them into four genera, which ought to be all ranged in the class of crustaceæ, and in the order of those whose gills or lungs are exposed to view. The most of those trilobites belong to the deepest, *i. e.* the most ancient of the stratifications of the soils which contain animal remains; they must therefore have been among the first animated beings; and in fact, as we approach the surface, we find crustaceæ more similar to those now contained in the sea; but the trilobites have disappeared entirely.

M. Cordier has published a memoir on the coal-mines of France, and on the progress which has been made in their working for the last twenty-five years. He proves that in this interval the produce has been more than quadrupled. This work, which is very important, is accompanied by an interesting map which points out the extent of our coal districts, the principal pits, and the direction of their various workings.

M. Vauquelin has again turned his attention to meteoric stones. It appears to him that a part of their silex is in combination with magnesia: there is also sulphur united to their iron, for it gives sulphuretted hydrogen gas on being dissolved in the acids. As to chrome, it seems to be isolated, and it shows itself sometimes in molecules so large as to remove every idea of combination.

Extracts from the Minutes of the Sittings of the Institute of March 4, 1816.

M. Arago made a verbal Report on the Voyage to the South Seas, drawn up by M. Louis Freycinet.

The work is divided into four books: the first bears the title of the Itinerary, and makes known the successive order of the operations; the second comprehends the nautical and geographical descriptions; the third is destined to the analysis of charts; and the fourth contains the general results of the observations, the measurements of inclination and declination of the magnetic needle, the remarks of various kinds made during the voyage, and the daily meteorological observations. The whole of the voyage is illustrated by thirty-two very fine charts. M. Freycinet himself engraved them on copper, and by a process peculiar to himself. The chapter in which he describes his method, as well as that which relates to the division of the scales, well merits the attention of engineers.

M. Biot read a Memoir drawn up by M. Pouillet and himself jointly, On the experimental determination of the diffraction undergone by simple or compound light when it passes between two parallel glass-bottle stoppers. The authors refer to measurements of fringes taken at different distances of the bottle stoppers on a piece of ground glass; and by constructing them they deduce the mode of separation of the rays, and the definitive division which the diffraction impresses upon them. According to these measurements, the ribbons which have the least deviated have their origin in the points of the interval the nearest to every stopper, and the most deviated have their origin nearest the central axis, both being deviated towards the stopper from which they were originally the most distant.

For every given separation of the stoppers, the incidence remaining always perpendicular to their interval, the deviations of the luminous particles of various natures are proportional to the length of their fits in the medium in which the light moves; and when the medium changes, all the other circumstances remaining the same, the absolute size of the deviations, and consequently the intervals of the fringes, vary also in proportion to the fits. The nature of the bodies which limit the medium do not change this law, whatever be the difference of their refrangent force. Stoppers of crown-glass opposed to each other form fringes in oil of turpentine, as metal stoppers would do if put in their place. Water at a heat of 37° of the centigrade thermometer forms fringes in water at 11°*.

Sittings of Monday, March 11, 1816.

In the name of a committee, M. Arago made a Report on the parallel mirrors which had been presented by Messrs. Richer.

These glasses, which are of very delicate workmanship, are employed, as is well known, in the construction of reflecting instruments, in that of tinned artificial horizons, which advantageously supply the place of the horizon of the sea in observations which are made at land, and in the formation of the guards which serve to keep the liquids, by the help of which we also sometimes procure the reflected images of the stars free from those agitations which the least breath of wind makes them undergo. For a long period the English artists have been in the habit of supplying the French sextant-makers with parallel mirrors. Messrs. Richer have succeeded in freeing us from this tribute. The glasses submitted to the examination of the Class were not less than four inches in diameter; rarely have they appeared to occasion angular deviations of three seconds:—a similar glass, which a well known artist had recently purchased in London, under the same circumstances, afforded greater deviations. The plain mirrors of Messrs. Richer, like those which come out of the workshops of that excellent optician M. Rebours, may therefore fairly be put on a level with all that has been done by foreigners of the same kind.

M. Ivart gave a verbal account of several works upon agriculture, presented to the class by Sir John Sinclair.

In the name of a committee, M. Giraud read a Report on a memoir of M. Dupin relative to the laying down of roads.

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* Messrs. Biot and Pouillet had undertaken these experiments at the end of the summer of 1815. On the 9th of October of the same year they announced to the Inssitute that they had discovered laws, according to which the phænomenon of diffraction was found to have the most intimate connexion with that of the coloured rings, and might be deduced from them numerically. They had added that these laws indicated also the species of modification extremely singular, according to which the light was diffracted. These indications are merely referred to the diffraction between two stoppers, the only ones which the authors had considered in their experiments.

This memoir is an application of descriptive geometry to some very important questions. We conceive that we may arrive at one point from another by an infinity of different roads; but these roads are not all equally favourable; the inclination, for instance, ought not to exceed in any point the limits given by experience, and after which the moving powers no longer act with advantage: the limit of slope varies according to the method of effecting the transport of articles, whether on the backs of men or by wheeled carriages. The mathematical theory of M. Dupin embraces all the elements of this question considered in the most general manner.

M. Brochaut read a paper on Gypsum. Messrs. Ramond and Brongniart are to examine it.

Monday, March 18.—A letter was read from Sir C. Blagdon. He announces that they are at this moment constructing in Cornwall steam-engines destined to work under a pressure of *seven* atmospheres. The trials already made seem to indicate that they will be productive of immense advantages. In order to determine under what circumstances steam-engines ought to produce the maximum of effect, keeping in view the quantity of coal used, it is necessary to know the relation which may exist at different temperatures between the elastic force of the steam and the quantity of caloric necessary for its production. Already had some French manufacturers ascertained that the increase of the elastic force is superior to that of the caloric employed; for they found an advantage in working their machines under pressures superior to that of the atmosphere; but the form of their boilers did not admit of their much exceeding this term. In England they have gone much further, by means of an invention of Mr. Woolf's, and which is combined in such a way as to employ the steam at very high pressures. It seems also that the steam-engines of this able engineer contain another useful modification, and which consists in the heated steam never being in immediate contact with the piston of the large cylinder, as it is in the common machines; in the latter case, as is well known, the piston soon loses its accurate adjustment, because the steam dissolves the greasy substances which lubricate it. In Mr. Woolf's apparatus the steam enters into a first cylinder, and there it presses on the surface of a column of oil, which it forces to enter into an interior cylinder, in which is the piston: it thus raises the piston without touching it, and lets it fall as soon as it is condensed. It is clear that this mechanism may be also applied on both sides of the piston, so as to produce *a double effect.*

XCVI. *Intelligence and Miscellaneous Articles.*

VENTILATION OF COAL-MINES.

We have already alluded to Mr. Ryan's improvements on this important subject, and we are happy to state that they have been rewarded by the Society of Arts, by a vote of one hundred guineas, and the gold medal, the highest premium ever voted by the Society.

Our readers already know that the most dreadful accidents which happen in coal-mines, and those of most frequent occurrence, arise from explosions occasioned by the accumulation of hydrogen gas, and the defective means hitherto adopted of carrying off that deleterious and highly inflammable substance.

Mr. Ryan's method of ventilation obviates all these dangers, and carries off every particle of the hydrogen gas the instant it is liberated from the coal. His first operation is to insulate the whole mine, or *field*, as it is technically called, by cutting round it a course or passage. This is what he calls his gas course; and it is always made of a size sufficient to carry off all the gas which would otherwise accumulate in the mine. Within the body of the mine itself, holes are cut of different diameters, entering into this gas course from the higher parts or roof of the mine. Between this gas course and the lower part of the upcast shaft of the mine, a communication is made, and the gas by its levity naturally ascends. Heat, however, is occasionally applied at the lower part of this shaft to accelerate the exit of the gas. Our philosophical readers must be aware that no mine whatever can produce the quantity of hydrogen which a gas course on this principle is capable of discharging. In fact, on the old system of ventilating by the labyrinth process, the atmospheric air remains at least twenty hours in a mine of common dimensions, during which it traverses a space of forty miles, and becomes every second more and more impregnated with hydrogen gas, and consequently increasing the danger throughout its whole passage through the workings of the mine. On Mr. Ryan's system, the inflammable gas, as fast as it flows from the workings, takes the nearest course to the upper gallery or reservoir for gas, whence it finds its way by the shortest course to the upcast shaft, through which it passes into the open air.

Mr. Ryan has introduced his valuable discovery with the happiest results into some of the most destructive and fiery coal-mines of Staffordshire and Worcestershire, and has obtained certificates of the most flattering description from numerous respectable and humane mine-owners. In some mines in Worcestershire, for instance, where daily explosions take place, and where none but the most desperate characters could be obtained as workmen, all alarm has subsided. Since the introduction of his system, the price of labour has fallen, and hundreds of valuable lives have been saved to the community.

We cannot, in short, pronounce a better eulogium on the author of this valuable discovery, than by presenting our readers with the short but eloquent address of the royal and illustrious President of the Society of Arts, when delivering the premiums awarded by the Society to this meritorious individual: "Mr. Ryan," said the Duke of Sussex, "in rising to present you with the rewards so justly voted you by the Society, they wish it to be understood, that they do not intend these rewards as any remuneration for your valuable discovery: for such remuneration you are to look to yourself—I mean, to the feelings of your own mind. But to mark their sense of your merits, the Society have voted you the highest premium ever given by them; and when I reflect on the personal risks and dangers you have run in bringing this invention to its present state—an invention which has already saved more than are now here present, and which promises to render the most lasting services not only to this, but to every country that may adopt it, I feel an increased source of satisfaction in being the organ of the Society on the occasion."

It may perhaps redound still further to Mr. Ryan's credit, to state, that no fewer than ten meetings of the Committee of the Society thoroughly investigated his plans and models previously to the final vote of the Society, so that every opportunity was afforded for that ample and free discussion of his merits which they received.

DEATH OF MR. THOMAS HENRY.

We are sorry to announce the death of the celebrated Mr. Henry, of Manchester. The distressing event is thus announced to the public in Wheeler's Manchester Chronicle of June 22:—"Died, on Tuesday the 18th instant, in the 82d year of his age, Mr. Thomas Henry, President of the Literary and Philosophical Society of Manchester, Fellow of the Royal Society of London, and member of several other learned societies both in this country and abroad. As a practical and philosophical chemist he had obtained a high and merited reputation. His contributions to that science, besides a small volume of Essays, and his translations of the early writings of Lavoisier, which he first introduced to the notice of the English reader, consist chiefly of Memoirs dispersed through the Transactions of the various societies to which he belonged, and relating both to those parts of chemistry that are purely scientific, and to those which have a connexion with the useful arts. On a subject intimately connected with the success of the cotton manufacture (the employment of mordants or bases in dyeing) 'Mr. Henry was the first,' to [illegible]

the

the words applied to him by a subsequent author, 'who thought and wrote philosophically.' In the introduction, too, of the new mode of bleaching, which has worked an entire revolution in that art, and occasioned an incomparably quicker circulation of capital, he was one of the earliest and most successful agents. In addition to the acquirements connected with his profession, he had cultivated to no inconsiderable degree a taste for the productions of the Fine Arts; he had obtained a knowledge of historical events remarkable for its extent and accuracy; and he had derived, from reading and reflection, opinions to which he was steadily attached, on those topics of political, moral, and religious inquiry, which are most important to the welfare of mankind. For several years past he had retired from the practice of medicine, in which he had been extensively engaged, with credit and success, for more than half a century; and from delicate health he had long ceased to take an active share in the practical cultivation of science. But possessing, almost unimpaired, his faculties of memory and judgement, he continued to feel a lively interest in the advancement of literature and philosophy. Retaining also in their full vigour those kind affections of the heart that gave birth to the most estimable moral qualities, and secured the faithful attachment of his friends, he passed through a long and serene old age, experiencing little but its comforts and its honours, and habitually thankful for the blessings with which Providence had indulged him."

RUSSIAN VOYAGE OF DISCOVERY.

The ship Suwarrow, Captain Lazaroff, belonging to the Russian East India Company, arrived at Spithead on Tuesday, June 18, whence she sailed on the 10th of March 1814, on a voyage of discoveries in the North Pacific Ocean, but more with a view to form two military and commercial establishments on the west coast of North America, namely, at the island of Rodiak, in lat. 55° N. long. 160° W. which is the nearest part of the American continent to the Russian establishment at Kamschatka, and upon a neck of land called California. From these they will be enabled to carry on their fur-trade with China with greater advantages; and their homeward-bound voyage, with the produce of China, will be likewise greatly facilitated. The Suwarrow has been so far as lat. 58° 50′ N. long. 190° 50′ E. She touched at Kamschatka. On the 10th of October 1814 she discovered an island in lat. 13° 10′ S. long. 163° 29′ W. It is about eight miles and a half long, and seven miles wide. Cocoa-nut trees and sea-fowl were fonnd upon it. The rocks around it appear formed of solid coral. It not being laid down in any chart, Captain Lazaroff named it after his ship, "Suwarrow Island."

Island." It appears from the great Vancouver's track, upon his published chart, that he must have passed this island in the night time. The Suwarrow has a valuable cargo of furs, which she took on board in Norfolk Sound, with many articles the produce of the coast of Peru. She lay two months at Lima. The cargo is not estimated at less than one hundred thousand pounds; and so prosperous has been the whole of the voyage, that she has not sustained the loss of even a rope or spar of any description since she sailed from Spithead. She has about fourteen rare animals on board—species of the Lama, Vigonia, and Alpaca. They are intended as a present to the Emperor of Russia. She has since sailed for St. Petersburgh.

LIST OF PATENTS FOR NEW INVENTIONS.

To George Bodley, of the city of Exeter, iron-founder, for an improved metallic engine to work either by steam or water, which he denominates "Bodley's improved metallic engine."—27th April 1816.—6 months.

To John Collier, of Windsor Terrace, in the county of Middlesex, engineer, for a machine for shearing woollen cloths.—1st May.—6 months.

To John Rangley, of Oakwell Hall, near Leeds, gent., for further improvements on his hydropheumatic engine, being a new or improved method of constructing and working engines or machines for lifting or raising of weights, turning machinery of all descriptions, drawing carriages on railways, and capable of being applied to all purposes where mechanical power is required.—4th May.—6 months.

To Richard Banks, of Hadley, in the parish of Wellington and county of Salop, engineer, for certain improvements on wheeled carriages.—4th May.—6 months.

To William Threadgold, of Farm Street, Berkley Square, surveyor and builder, for his machine or apparatus to prevent obstructions to the passage of smoke in and through chimneys.—4th May.—6 months.

To Robert Copland, of Liverpool, in the county of Lancaster, merchant, for his certain means of effecting a saving in the consumption of fuel.—4th May.—6 months.

To Benjamin Rotch, late of Castle Hall, Milford Haven, in the county of Pembroke, but now of the city of Bath, gent., being one of the people called Quakers, for a flexible elastic horse-shoe for the purpose of allowing the foot of the horse its natural motion when shod.—11th May.—2 months.

To Jean Samuel Pauly, of Knightsbridge, for certain improvements in the construction and use of fire-arms.—14th May.—6 months.

METEORO-

METEOROLOGICAL JOURNAL KEPT AT BOSTON, LINCOLNSHIRE.

[The time of observation, unless otherwise stated, is at 1 P.M.]

	Age of the Moon.	Thermometer.	Barometer.	State of the Weather.	Modification of the Clouds.
1816.					
May 15	18	58 0	30·05	rain	
16	19	65·0	29 97	fair	
17	20	64 0	29 90	very fine	
18	21	50 0	29 95	fair	
19	22	54 5	30·02	do	
20	23	63 0	30 06	fine	
21	24	58 0	30·10	very fine	
22	25	55 5	30·10	fine	
23	26	58·5	30 15	very fine	
24	27	50 0	30 0	rain	
25	28	57 0	29 91	do	
26	29	54·0	30 20	fair	
27	new	54 0	30·24	rain	
28	1	64·5	30 25	fair	
29	2	65·0	30·10	do	
30	3	64·0	30·12	do	
31	4	58 0	30·0	do	
June 1	5	63·5	30·15	very fine	
2	6			fair	gale from the west.
3	7	60·0	30·15	do	
4	8	55 0	30·0	do	
5	9	49 0	29 75	rain }	a strong N.W. wind.
6	10	45·0	30·0	do }	
7	11	59·0	29·78	do	
8	12	51·0	29·54	do	
9	13	53·0	29·40	violent storm of hail and rain.	
	eclipsed				
10	full	53·0	29 81	showery	
11	15	54·5	30 10	fine	
12	16	61·0	30·23	very fine	
13	17	69·0	30 15	very cold in the morning; came on sultry at noon, and in the evening rain.	
14	18	57·0	30·18	rain	

METEOROLOGICAL TABLE,

BY MR. CARY, OF THE STRAND,

For June 1816.

Days of Month.	Thermometer.			Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
	8 o'Clock, Morning.	Noon.	11 o'Clock, Night.			
May 27	50	55	58	30·10	0	Small rain
28	55	63	55	·11	39	Fair
29	53	66	55	29·97	54	Fair
30	55	69	55	·85	50	Fair
31	56	73	56	·82	67	Fair
June 1	56	73	61	30·02	68	Fair
2	63	71	54	29·99	54	Rain
3	56	68	54	30·08	69	Fair
4	55	68	55	29·97	60	Fair
5	54	60	45	·69	52	Showery
6	49	52	50	·85	47	Cloudy
7	49	60	54	·65	0	Rain
8	55	60	52	·72	0	Rain
9	52	57	47	·25	42	Stormy
10	48	58	50	·85	40	Showery
11	50	58	52	·95	48	Fair
12	55	67	54	30·06	42	Fair
13	60	65	52	·01	44	Fair
14	55	55	50	29·96	0	Rain
15	50	56	50	·99	39	Cloudy
16	51	55	52	·99	42	Cloudy
17	51	60	55	·98	46	Fair
18	55	69	55	30·02	53	Fair
19	55	67	55	·07	62	Fair
20	56	67	55	·06	60	Fair
21	59	72	56	·06	66	Fair
22	57	69	56	.01	63	Fair
23	60	67	51	29·85	54	Showery
24	56	62	55	·86	50	Cloudy
25	55	72	61	·92	68	Fair
26	60	60	53	·70	0	Rain

N.B. The Barometer's height is taken at one o'clock.

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END OF THE FORTY-SEVENTH VOLUME.

ALERE FLAMMAM.

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